# THE JOURNAL
OF
# "The Royal Aeronautical Society"
with which is incorporated "The Institution of Aeronautical Engineers"
Edited for the Council by Captain J. LAURENCE PRITCHARD, Hon. Fellow

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## NOTICES

### *Annual General Meeting*

The attention of members is called to the report of the Annual General Meeting on pages 245-6.

### *Elections*

A meeting of the Grading Committee was held on Wednesday, 14th March, at 4.30 p.m. Their report was presented to the Council and the following decisions recorded:—

*Elected to Associate Fellowship.*—George Purves Douglas, D.Sc., Royal Aircraft Establishment; Sydney George Ebel, Experimental Department, Handley Page, Ltd.; John Henry Lower, Experimental Department, Short Brothers; Ewart Guy Milburn Neville, M.A., B.Sc., No. 1 School of Technical Training, R.A.F., Halton Camp.

*Transferred to Associate Fellowship from Student Membership.*—Frederick Francis Crocombe, A.C.G.I., D.I.C., B.Sc., Technical Staff of Fairey Aviation Co.

*Elected to Associate Membership.*—Wilfred Barnard, Senior Draughtsman, Blackburn Aeroplane and Motor Co., Ltd.

*Elected to Associateship.*—Arthur Edward Bingham, Charles Chapleo, Clifford Godfrey Rogers.

*Student.*—F. R. B. King.

*Companion.*—I. T. P. Hughes.

### *Branch Notes*

Branch Notes appear on page 336.

### *Taylor Medal*

Mrs. Taylor has most generously offered to continue the Gold Medal given annually by her late husband, Mr. G. A. Taylor. The medal was awarded to Captain Sayers in 1926 and to Captain Barnwell in 1927 for their papers before the Institution of Aeronautical Engineers. The Council desire to record their appreciation of Mrs. Taylor's offer.

### *Technical Information Index*

A card index is in the course of formation at the Society's offices, giving references to articles, papers and books on all aspects of the science of aeronautics and its allied sciences. The index already contains many thousands of references and it is hoped, in time, to make it one of the biggest aeronautical indexes extant. The use of this index is free to all members of the Society.

The following is a list of publications, etc., covered at present by the index:—

*The Aeroplane*, January, 1928, to date.
*Flight*, ,, ,,
*Airways*, ,, ,,
*Aero Digest*, ,, ,,
*Aviation*, ,, ,,
*Western Flying*, ,, ,,
JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY, 1897 to date.
Books in Library of the Royal Aeronautical Society.
All publications of the Aeronautical Research Committee, 1912-1928.
,, ,, U.S.N.A.C.A., 1915-1925.
Light Alloys Report, 1921.
U.S.N.A.C.A. Technical Notes, No. 1 to date.
The Reports of Toronto University.
Air Ministry Abstracts from the Scientific and Technical Press.

Other sources of reference will be added to the list from time to time.

### *Lecture Programme*

April 12th.—Dr. Hele-Shaw on "The Variable Pitch Propeller."
April 26th.—Herr Dornier on "Flying Boats."
May 10th.—Mr. B. N. Wallis on "The Design and Construction of Modern Rigid Airships."
May 30th.—Wilbur Wright Memorial Lecture, by Mr. Handley Page.

### *Forthcoming Arrangements*

Thursday, April 12th, 6.30 p.m.—At the Royal Society of Arts, 18, John Street, Adelphi, W.C.2: "The Variable Pitch Propeller," by Mr. H. S. Hele-Shaw, D.Sc., LL.D., F.R.S., M.Inst.C.E., M.I.Mech.E., M.I.Chem.E., and Mr. T. E. Beacham, B.Sc., A.C.G.I., A.M.Inst.C.E., A.M.I.Mech.E.

Tuesday, April 17th, 5.30 p.m.—Council Meeting.

Thursday, April 26th, 6.30 p.m.—At the Royal Society of Arts, 18, John Street, Adelphi, W.C.2: "Flying Boats," by Herr C. Dornier.

Thursday, May 10th, 6.30 p.m.—At the Royal Society of Arts, 18, John Street, Adelphi, W.C.2: "The Design and Construction of Modern Rigid Airships," by Mr. B. N. Wallis.

J. LAURENCE PRITCHARD, *Secretary*.

## OBITUARY

### G. A. TAYLOR, MEMBER

It is with very great regret that the council received news of the death on January 20th, in Australia, of Mr. George Augustine Taylor.

Mr. Taylor was one of the best known figures in Sydney, where he was born in 1872. He was one of the earliest and most enthusiastic workers in the development of wireless telegraphy, and as early as 1910 he succeeded in transmitting messages from one moving train to another. Mr. Taylor took an active interest in aviation and was a Member of the Institution of Aeronautical Engineers. In 1926 he inaugurated the Taylor Gold Medal, to be awarded annually to the member of the R.Ae.S.I. who submits or reads the most valuable paper during a session.

# CORRESPONDENCE

## THE ORIGINAL WRIGHT BIPLANE

[From time to time considerable discussion has arisen, chiefly due to the attitude of the Smithsonian Institution, after the death of Professor Langley, Secretary of the Institution. A campaign was begun to take from the Wrights credit, which had up to then never been disputed, of having designed and made from their own independent research the first machine to fly in the air. The attitude of the Smithsonian Institution has been dealt with in the JOURNAL of July, 1912; July-September, 1916; December, 1921; and March, 1927, by Mr. Griffith Brewer, and the attitude of Mr. Orville Wright himself is explained at length in *U.S. Air Services* for March, 1928. In view of the importance of combating the unfounded and ungenerous attacks on the achievement of the Brothers Wright, the circular notice recently received by the Editor is published here, together with a letter received from Mr. Griffith Brewer, to whom the notice was submitted for comment.—EDITOR.]

*Offer of Secretary C. G. Abbot, of Smithsonian Institution, to Orville Wright, March 4, 1928.*

Elected in January to be Secretary of the Smithsonian Institution, I inherited a knotty problem, for February brought me face to face with the Wright controversy. On February 13 I wrote to Mr. Orville Wright: "It would be a matter of great gratification to me and to all our countrymen if now, or even at a later time, you should see your way to depositing it (the Kitty Hawk machine) here."

On his part Mr. Wright has said that both he and his late brother wished to deposit the plane in the United States National Museum, but that it is now sent "to the British National Museum because of the hostile and unfair attitude shown towards us by the officials of the Smithsonian Institution." It is stated that the plane is still subject to recall. Since both Mr. Wright and the Smithsonian desire it, there remains only to come to just terms.

The people of the United States, who support the National Museum, are vitally interested. They ardently desire that an object of such pride to all Americans as the Wright machine of 1903 should join the National Valhalla of aeronautics where rest so many planes that have made aviation history.

I will not again present from the other point of view the questions recently raised by Mr. Orville Wright and his friends, for fixed opinions would remain unchanged. Braving warnings that whatever I now say will be misconstrued, I ask a fair hearing for the following offer.

To make understood what I now propose, I must explain that the Langley machine of 1903 is now on exhibition in the National Museum, with a label attached which was prepared in 1925, according to the advice of a committee. Two gentlemen from outside the Smithsonian, namely, Dr. Joseph S. Ames, of Johns Hopkins University, Baltimore, and Admiral David W. Taylor, United States Navy, who are now Chairman and Vice-Chairman respectively of the U.S. National Advisory Committee for Aeronautics (of which body Mr. Orville Wright is also a member), formed the committee. At the late Secretary Walcott's request they examined the records of the Langley machine, including much

unpublished correspondence, took testimony of experts, and presented a report which was given to the Press on June 9, 1925. The label as revised to accord therewith now stands as follows:—

LANGLEY AERODROME.

THE ORIGINAL LANGLEY FLYING MACHINE OF 1903, RESTORED.

IN THE OPINION OF MANY COMPETENT TO JUDGE, THIS WAS THE FIRST HEAVIER-THAN-AIR CRAFT IN THE HISTORY OF THE WORLD CAPABLE OF SUSTAINED FREE FLIGHT UNDER ITS OWN POWER, CARRYING A MAN. THIS AIRCRAFT SLIGHTLY ANTEDATED THE MACHINE DESIGNED AND BUILT BY WILBUR AND ORVILLE WRIGHT, WHICH, ON DECEMBER 17, 1903, WAS THE FIRST IN THE HISTORY OF THE WORLD TO ACCOMPLISH SUSTAINED FREE FLIGHT UNDER ITS OWN POWER, CARRYING A MAN.

(There follows in small type accounts of the investigations of Langley and of his machines, too long to quote here.)

I believe that label to be just, as do my colleagues, and cannot think anybody would wish us to recant falsely. For the sake of the public, I make the following offer.

If Mr. Wright will openly state in a friendly way that he appreciates that the Smithsonian Institution honestly believes that the Langley machine of 1903 was capable of sustained free flight under its own power, carrying a man, and that it now removes that public statement, not in confession of error, but in a gesture of goodwill for the honour of America; then I am willing to let Langley's fame stand on its merits and to reduce the Langley label to this simple statement: "Langley Aerodrome.—The Original Langley Flying Machine of 1903, Restored."

I will do this, of course, provided Mr. Wright will deposit the Kitty Hawk machine in the National Museum, where it has always been wanted, where it will have the place of honour due to it, where the label will state that it was the first heavier-than-air craft in the history of the world to accomplish sustained free flight under its own power, carrying a man, and where it will be preserved inviolate to the Wrights' perpetual honour.

### *Mr. Griffith Brewer's Comment*

The Secretary,
The Royal Aeronautical Society,
7, Albemarle Street, W.1.

*20th March*, 1928.

DEAR MR. PRITCHARD,—I have to thank you for giving me the opportunity of commenting on Secretary C. G. Abbot's offer to Mr. Orville Wright, which has already been given wide publicity in the American Press. If similar publicity had been given to the report of Dr. Ames and Admiral Taylor, my report to the Royal Aeronautical Society of the change in the label on the Langley machine would not have been delayed until some months after the statement on the Langley label had been changed from a statement of fact to one of opinion.

The present "offer" contains the first intimation of any inquiry based on testimony having taken place, and as readers of the JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY would naturally suppose that both sides in such an inquiry would have been invited to give evidence, I would say that until now I have never heard of any such inquiry having taken place.

On the merits of the "offer," little need be said. It amounts to an invitation to Mr. Orville Wright to make an insincere statement, in return for which

Dr. Abbot would withdraw the incorrect description on the Langley machine. Certainly Dr. Abbot only inherited this incorrect labelling of a Museum exhibit, and it has already been partly corrected. But why not complete the correction for the sake of truth, without bargaining?

Yours faithfully,
GRIFFITH BREWER.

## AIR OR GROUND ENGINEER?

"Briarley," Hendford, Yeovil,
*19th March*, 1928.

The Secretary,
The Royal Aeronautical Society,
7, Albemarle Street, W.1.

DEAR SIR,—In view of the Society being the only body representing those engaged in the profession of aeronautics, I am writing to suggest that consideration might be given by the Society to the terminology used in describing those engaged in the profession.

To illustrate my meaning, take the case of the term "Ground Engineer," this is used at the present time to describe one who has a knowledge of aircraft construction, and who is, in fact, an Aircraft Engineer. It is a source of irritation to such men to be termed "Ground Engineers," particularly as frequently, nowadays, it is necessary for them to fly as passengers in order to ascertain for themselves whether or not certain portions of the aircraft are functioning satisfactorily. I beg to suggest that the Society might with advantage take up this point with the Air Ministry, while it is still possible to make a change in such designations.

I understand that the B.E.S.A. are considering the revision of their Glossary of Aeronautical Terms, and in this connection I venture to suggest that the Society might consider the following terms, which, if approved, could be submitted to the B.E.S.A. for standardisation.

*Aeronautical Engineer.*—One who is qualified to design aircraft, and to supervise the construction of aircraft.

*Aircraft Designer.*—One who is qualified to design aircraft.

*Aircraft Engineer.*—One who is qualified to supervise the construction of aircraft.

*Air Engineer or Aerial Engineer.*—One who is carried on an aircraft for the purpose of doing running repairs, and for maintaining the machinery in working order. (This term is analogous to "Marine Engineer.")

This list eliminates the obnoxious term "Ground Engineer."

I am not sure if it is within the province of the Aeronautical Society to deliberate on this matter, but if it is not, possibly you could have the question raised in the proper quarters?

Yours faithfully,
R. C. TAYLOR,
*Associate Fellow, R.Ae.S.I.,*
*Licensed Ground Engineer,*
*Categories A, B and C.*

## INFORMAL DISCUSSION AND DINNER

Royal Naval College, Greenwich,
*24th February*, 1928.

DEAR COLONEL SEMPILL,—Many thanks for your letter of 16th February. I am sorry to have been so long in answering it, owing to rather a heavy week.

It is difficult to reduce to words any "general impressions" of the discussion the other night, but one point which did strike me very much as an onlooker was the wonderful—I might almost say magical—spirit of good comradeship which breathed through the whole evening. Everyone was so genuinely out for the good of the show—no one had even the suspicion of an axe to grind; it was aviation that mattered and nothing else. It really was very striking, and I came away with the feeling that although aviation might not—and indeed does not—receive anything approaching the recognition and support it is entitled to expect from the country in general, still its future development was in the hands of a band of brothers who were heart and soul in the business, and to whom that business was the great big thing that mattered.

This is, I fear, crudely expressed, but it is what I felt.

As regards the immediate subject of the debate, my impression rather was that the feeling of the meeting was that aeroplanes were the instruments of the immediate present, but that, before long (five years or so), flying boats would have definitely established their superiority for imperial communications.

Believe me, yours very truly,

(Sgd.) RICHARD WEBB.

P.S.—How Hinkler must have longed for a flying boat during his last "trip" from Bima to Port Darwin.

[Admiral Sir Richard Webb was the guest of honour at the Informal Discussion on Seaplanes *v.* Aeroplanes in Imperial Communications, held at the Engineers Club on February 14th, 1928.—EDITOR.]

# ANNUAL GENERAL MEETING

The Sixty-Third Annual General Meeting of members of the R.Ae.S.I. was held in the Library at 7, Albemarle Street, W.1, at 5.30 p.m. on Tuesday, March 27th, 1928, Colonel the Master of Sempill, President of the Society, being in the chair.

The following members were present:—Captain P. D. Acland, Flt.-Lieut. R. E. H. Allen, Mr. R. M. Balston, Colonel Belaiew, Mr. M. L. Bramson, Mr. G. Brewer, Mr. Dendy Marshall, Mr. S. H. Evans, Mr. E. J. Fearn, Mr. J. D. Frier, Mr. R. C. B. Hendy, Captain F. T. Hill, Mr. J. E. Hodgson, Mr. C. D. Holland, Mr. N. J. Hulbert, Major Kennedy, Captain Lamplugh, Mr. M. Langley, Major Low, Mr. W. O. Manning, Mr. G. Merton, Lieut.-Col. M. O'Gorman, Mr. J. S. L. Oswald, Mr. F. Handley Page, Prof. C. H. Powell, Flt.-Lieut. R. Chevallier Preston, Mr. T. B. Ringwood, Colonel the Master of Sempill, Dr. A. P. Thurston, Sir Vyell Vyvyan, Mr. H. E. Wimperis, Mr. L. A. Wingfield, and Mr. R. McKinnon Wood.

The Notice convening the Meeting was read by the Secretary.

The Chairman drew attention to the fact that this was the first Annual General Meeting since the amalgamation, and stated that the amalgamation had gone through smoothly and was working well in every way. He pointed out that the lectures had been better attended. The number of lectures held had increased from twelve during the 1924-25 session to twenty in the present session, not including those held by the Branches. The reading matter in the Journal had grown from 700 pages in 1924 to 1,170 in 1927.

After giving a summary of the position of the Branches formed under the 1925 scheme, and including the Australasian and Manchester Branches formed under the auspices of the Institution of Aeronautical Engineers, the Chairman expressed appreciation of the action of the Guggenheim Fund, the Air Ministry, the Society of British Aircraft Constructors and the Society of Motor Manufacturers and Traders, for their generous donations to the Society's funds, and thanked Captain Acland, Mr. Wimperis and Mr. Simms for the work they had done in this connection. The donations received since 1925 are given in the following table:—

| | | |
|---|---|---|
| 1925. | Air Ministry ... ... ... ... ... | £250 |
| | Society of British Aircraft Constructors ... | £250 |
| | Total ... | £500 |
| 1926. | Air Ministry ... ... ... ... ... | £250 |
| | Society of British Aircraft Constructors ... | £250 |
| | Total ... | £500 |
| 1927. | Air Ministry ... ... ... ... ... | £250 |
| | Society of British Aircraft Constructors ... | £250 |
| | Guggenheim Fund ... ... ... ... | £1,528 |
| | Total ... | £2,028 |

This year the Guggenheim Fund had repeated their generous donation of 5,000 dollars, and the Society of Motor Manufacturers and Traders had given £250. The Chairman regretted that Mr. F. R. Simms was not present to hear the thanks of the meeting to him for his good offices in this connection.

Notwithstanding the increase of membership shown in the Council's Report, the Chairman urged upon every present member of the Society to encourage others to join. He pointed out that there were many hundreds of qualified men in the Royal Air Force, the Air Ministry and the industry who were not yet members. Their accession to the Society would go largely towards making it independent of outside support, and furthermore would enable the Council to put in hand certain schemes which otherwise must rest in abeyance.

The Chairman reminded those present at the meeting that in June would occur the twentieth anniversary of the first controlled flight in this country by an Englishman, Mr. A. V. Roe, and later in the year would come the twenty-fifth anniversary of the Wright brothers' first flight. Schemes for celebrating these events were on foot.

After thanking the Staff and Major Kennedy, the Honorary Treasurer, for their work, the Chairman informed the meeting that the legacy of £500 bequeathed to the Society by the late Sir Mackenzie Chalmers had been received and added to the Endowment Fund, which now stood at £1,047. He pointed out that more ought to be done to accelerate the growth of this fund.

Major Kennedy, the Honorary Treasurer, gave a short account of the financial position of the Society.

The Council's Report for the year 1927-1928, and the Balance Sheet and Accounts, as published in the March issue of the Journal, were adopted.

In reply to a question regarding the various Memorial Funds administered by the Society, Mr. Wingfield said that the whole question of the trusteeship of these funds would be considered by the Council at their next meeting.

Messrs. Price, Waterhouse were re-elected Auditors for the ensuing year.

A proposal that future Annual General Meetings should be held at a time more suitable for the attendance of the bulk of the membership was referred to the Council for future action.

Mr. R. M. Balston, Mr. J. D. Frier and Sir Vyell Vyvyan were appointed scrutineers of the ballot for the election of new members to the Council, with Major Kennedy to supervise. As a result of the ballot the following members were declared duly elected to serve on the Council:—Captain P. D. Acland, Mr. M. L. Bramson, Mr. Griffith Brewer, Major J. S. Buchanan, Captain F. T. Hill, Captain A. F. Lamplugh, Major A. R. Low, Mr. W. O. Manning, Lieut.-Colonel J. T. C. Moore-Brabazon, Mr. F. Handley Page, Mr. H. E. Wimperis, Mr. L. A. Wingfield, and Mr. R. McKinnon Wood.

A vote of thanks was accorded to the scrutineers for their services, and to the Chairman, and the meeting closed.

# PROCEEDINGS

## THIRD MEETING, FIRST HALF, 63RD SESSION

The Third Meeting of the Sixty-Third Session of the Royal Aeronautical Society, with which is incorporated The Institution of Aeronautical Engineers, was held in the Theatre of the Royal Society of Arts, 18, John Street, Adelphi, W.C.2, on Thursday, October 20th, 1927, when Mr. M. L. Bramson, A.F.R.Ae.S., M.I.Ae.E., read his paper on "Safety Devices for Aircraft." Colonel the Master of Sempill (Chairman of the Society) was in the chair.

Colonel THE MASTER OF SEMPILL: Mr. Bramson's name will be well known to you in connection with his activities with Major Savage in sky-writing, and more particularly as the inventor of the Anti-Stall Gear, which he produced in the early spring of 1925, and which is now being introduced into the Royal Air Force.

I should like to say that Mr. Bramson, as a prominent member of the Institution of Aeronautical Engineers and the Royal Aeronautical Society in the old days, did a great deal to bring those bodies together, and we are all very much indebted to him for his diplomatic work in that direction.

Mr. Bramson came over to this country shortly before the war, and during the early part of the war was working with Mr. Constantinesco.

## SAFETY DEVICES FOR AIRCRAFT

BY M. L. BRAMSON, A.C.G.I., A.F.R.AE.S., M.I.AE.S.

### *Introduction*

The time has come when aeroplanes may be considered as having definitely passed the stage in which their usefulness has been severely limited and hampered by their performance. The one real great limitation to their usefulness is now represented by the dangers, such as they are, inherent, or so far associated with, their use.

There are two distinct conceivable methods of minimising or eliminating those dangers. One would consist in inventing a new kind of flying machine radically different from our conception of an aeroplane (such as, for instance, the La Cierva Autogyro), and the other method consists broadly in tackling the different kinds of danger, one by one, and endeavouring to overcome them by means of some device specifically designed for that purpose.

It is the object of this paper to describe (so far as may be necessary) and to discuss critically all such safety devices as appear likely at the present moment in the present state of the art to diminish the number of aircraft accidents, fatal or otherwise, or to increase the number of flights which may be carried out in safety. Before, however, proceeding to any such descriptions or discussion, it is almost unavoidable, for the sake of clarity, to interpose a brief analysis of the dangers in question.

### *Analysis of Flying Dangers*

In endeavouring to establish what might be termed a qualitative analysis of flying dangers, a number of necessary sub-divisions suggest themselves.

### *Classification of Accidents*

It is usual in this country to classify the causes of accidents under the following main headings:—

1. Errors of judgment.
2. Engine or installation failures.
3. Weather.
4. Other causes.

In order to assess the relative importance of all these sources of accidents and so render the analysis in a sense quantitative, it would be necessary to have available complete accident statistics, together with the reports of the Accident Investigation Department of the Air Ministry. The only such statistics which are available are those dealing with civil aviation. The number of civil aviation accidents, however, is fortunately so small per thousand hours flown and the number of hours flown is, unfortunately, likewise so small, that these data hardly amount to statistics.

Military flying accident statistics are not available to the public, which fact has formed the subject of some controversy in and out of Parliament. Incidentally, if one may be permitted to contribute that controversy, one would express it as a most emphatic opinion that the policy of keeping secret, and therefore apparently mysterious, the causes of every accident whilst of necessity publishing in each case the fact that an accident has occurred, is short-sighted and exceedingly harmful to the progress of flying in this country. The alleged grounds for such a policy are the risk of unjustly blaming the material produced by some particular manufacturer.

It cannot be said too strongly that knowledge so valuable and so dearly bought with the lives and treasure of the community belongs of right to the community as a whole, and not to a department of the Air Ministry. It is not suggested that that department is not making most intelligent use of that knowledge, but there are active, inquiring and constructive minds outside the Air Ministry who should be assisted in their efforts to reduce or eliminate the outstanding weaknesses of aeroplanes by having all available facts put before them.

However, failing such statistics from this country, it may be of interest to give here the corresponding French statistics, for which I am indebted to an article by Monsieur H. Brunat in *La Technique Aeronautique* of May and June, 1926.

During 1923-24-25, out of 100 accidents to French civil, military and naval aeroplanes, the causes have been in the following almost constant proportions:—

| | | | |
|---|---|---|---|
| 54 | have been | due to | faulty airmanship (" error of judgment "). |
| 22 | ,, | ,, | engine failure. |
| 11 | ,, | ,, | defects in aircraft (structural). |
| 5 | ,, | ,, | weather. |
| 8 | ,, | ,, | cause unknown. |

The seriousness of these accidents has always depended directly on their causes and, in fact, out of 100 killed and 100 injured persons:—

| | | | | | |
|---|---|---|---|---|---|
| 62 | were killed | and | 63 | injured | as a result of faulty airmanship. |
| 15 | ,, | ,, | 6 | ,, | due to defects of material. |
| 7 | ,, | ,, | 8 | ,, | due to weather. |
| 5 | ,, | ,, | 19 | ,, | due to engine failure. |
| 11 | ,, | ,, | 4 | ,, | due to unknown causes. |

It is probable that these figures differ considerably from those which would be shown by corresponding British statistics. For the purpose of this paper, however, it is more important to establish that a particular kind of accident is not, in fact, negligible than to establish its actual or relative importance.

We must now endeavour to determine the basic causes of these various accidents in their apparent order of importance, and then review the devices so far developed to eliminate them.

### *"Errors of Judgment"*

Accidents referred to as due to "errors of judgment" are usually associated with unintentional stalling. The problems of stalled flight have been dealt with authoritatively before this Society by Professor Melville Jones in his recent paper, and I shall therefore deal with that aspect as briefly as my subject permits.

When the wings of an aeroplane reach the angle of incidence corresponding to the maximum lift coefficient of the machine, it is said to be stalled. Assuming a dead calm and that the pilot does not move his controls, an ordinary aeroplane then behaves as follows:—Due to the high rate at which drag increases with incidence near the stall, the machine rapidly decelerates with corresponding rapid decrease of lift. The weight of the machine being no longer balanced by the lift, a downward acceleration occurs, the effect of which is to curve the path of (the centre of gravity of) the machine downwards, thereby still further increasing the angle of incidence. The centre of pressure is thereby moved backwards so far behind the centre of gravity that the resulting nose-diving couple cannot be overcome by the elevator and tailplane reaction. The machine therefore nose-dives with or without auto-rotation.

If, due to gusts, the lateral controls are used at or near the stall, the depressed aileron, instead of increasing the lift, increases the drag on that side and so hastens an unsymmetrical stall, the rotation so started usually being continued during the nose-dive.

If the necessary height is not available, these evolutions generally end in a fatal crash.

The efforts to eliminate this state of affairs have followed a number of different channels. Apart from the auto-gyro which, as already mentioned, falls outside the scope of this paper, devices have been developed—

(*a*) To make stalling impossible.
(*b*) To improve control near the stall.
(*c*) To make involuntary stalling impossible whilst retaining the facility for stalling deliberately.
(*d*) To dissociate loss of control, nose-diving and spinning from the stall and so render it relatively harmless.

Of these, the latter is undoubtedly the best and most difficult solution.

Taking them in the order mentioned—

### *(a) The Rohrbach Device*

The object of Dr. Rohrbach's invention is to prevent stalling. He achieves this object by means of a horizontal vane stably suspended in the air-stream at the tail of the machine and adapted to control what might be called the gear ratio between the control column and the elevator in such a manner that the tailplane movement produced by a given movement of the control column diminishes as the incidence increases. In fact, near the stalling incidence, the elevator controls become inoperative, and it is therefore impossible to stall the machine. This arrangement has the following distinct disadvantages. When flying near the stalling incidence and therefore at low air speed, maximum effectiveness of all the controls is desirable, whereas with this arrangement the fore and aft control is then reduced to nothing. Moreover, in gliding slowly in to land, for instance, the pilot may unknowingly be so near the stalling incidence that the Rohrbach gear has rendered his fore and aft controls inoperative. When the time comes to flatten out, he has not the wherewithals and some sort of crash would appear unavoidable.

### *(b) The de Havilland Differential Aileron Control*

This device, which is now extensively used and therefore too well known to need describing, overcomes the tendency to start a spin due to the drag of the depressed aileron. By this device, lateral control is obtained by a decrease of lift on the appropriate side, rather than by a combined increase and decrease of lift on the respective appropriate sides of the machine.

Whilst considerably reducing the tendency to lose control at angles just below the stalling incidence, this device can in no sense be regarded as a remedy for the stalling danger.

The fitting of abnormally large rudder and fin surfaces may similarly help to improve control, but this is a matter which need not be discussed here in detail as it does not refer to any particular device.

### *(c) The Savage-Bramson Anti-Stall Gear*

An important fact stands out from the available evidence relating to stalling accidents, namely, that the stall in every case is inadvertent. This has led to the conclusion that if only the pilot could be definitely and infallibly warned at the critical moment, he would not stall his machine and the accident would either be avoided entirely or else take the ordinary and much less dangerous form of a forced landing on unsuitable ground.

These considerations led to the design of the anti-stall gear. It consists of two distinct units—the stall detector and the warning unit. The stall detector is a horizontal vane facing the air stream, statically balanced on a horizontal pivot and unstable with reference to the air stream. It has a free movement of about 7° and the stops are so positioned that at normal flying angles the air pressure on the vane is directed downwards; the vane therefore rests against the lower stop. On reaching the angle of incidence (say 14° for a normal machine), at which the pilot ought to be warned that he is about to stall, the lift on the vane changes sign and the vane flicks up against the top stop through an angle of 7°.

This movement of the detector is utilised to operate a pneumatic relay which, in turn, applies pressure to a piston attached to the control column. This pressure represents a forward force of 7 to 20 pounds at the pilot's hand, depending on the size of the machine, and can easily be overcome by him if he so desires; but it is impossible suddenly to provide that extra pull backwards without knowing it.

The pilot is therefore effectively warned, and if the circumstances are such that a stall would obviously be dangerous, he will comply with the hint and put the stick forward. When the incidence has decreased by 7° the detector vane flicks down again, and the pressure on the piston in the warning unit is released to atmosphere. The pilot then feels the sudden disappearance of the force previously applied and knows that he is well away (over 7°) from the stalling incidence.

This device, it will be seen, tackles the human, the physiological aspect of the problem, and does not in any way affect the aerodynamic characteristics of the machine. As it can be fitted in a few hours to any type of machine, it would, I think, be feasible to save a large number of lives by its use; at any rate, during the period which must elapse before the device, to which I am about to refer, becomes universal.

### *(d) The Handley-Page Slot*

The aerodynamic characteristics of the slotted wing are, I believe, familiar to this audience. The effect of the slot is to delay or prevent the breakdown of the airflow round the wing which, with an ordinary wing section, occurs at the usual stalling incidence. The effect of this is twofold. The lift coefficient

continues to increase with incidence, up to a maximum value, which may be as much as 100 per cent higher than the maximum coefficient with no slot. Further, the backward movement of the centre of pressure is likewise delayed or prevented.

Originally these useful properties of the slotted wing were utilised either to increase the speed range for a given wing loading, or to increase the wing loading for a given stalling speed.

For this purpose the wings of the machine were fitted with slots along the full length of their leading edges and the slots could be opened and closed by the pilot in flight. The idea was to take off and land with the slots open and to cruise with the slots closed.

With this arrangement it will be realised that the machine behaves exactly like an ordinary aeroplane when cruising with the slots closed and no protection is afforded against an unintentional stall in these conditions. Such protection is, however, partially effected during the take-off and landing operations and, to that extent, the arrangement may be regarded as a safety device.

A further development which resulted, I believe, from co-operation between Mr. Handley Page and the technical staff at the Royal Aircraft Establishment, Farnborough, provided separate slot control for the starboard and port wings and coupled the slot operating gear to the aileron controls in such a manner that the slot opened on the wing whose aileron was depressed. Some exceedingly good results, as regards maintenance of lateral control at or near the stall, were, I believe, obtained with this arrangement, especially when combined with large rudder and fin surfaces.

It is clear that when a machine is flying near the stalling incidence, the depression of an aileron is equivalent to an increase of incidence on that wing. Normally, therefore, a breakdown of the airflow would occur with resulting increased drag and decreased lift; but the simultaneous opening of the slot prevents the breakdown of the airflow and so causes the depression of the aileron to have the desired effect, namely, that of increasing the lift on that wing.

A further, and I think remarkable, development has taken place quite recently and, although by the courtesy of Mr. Handley Page I have had it shown and explained to me, it has at the same time been impressed upon me that it is not yet ready for what might be called "general release." I am therefore unable to make more than a superficial reference to this latest development of the Handley Page slot.

I think I am allowed to say that the operation of the slot mechanism is entirely automatic and that, in all circumstances, the slot opens and closes exactly as and when it should do so and without the intervention of the pilot.

It is my personal view, which I think will be confirmed when the full details become available, that this latest application of the Handley Page slot will prove to be one of the most important aerodynamic advances achieved during the last few years.

### *Engine Failure*

There are three distinct dangers that may be associated with engine failure.

(1) It may lead to a forced landing on unsuitable ground.

The only complete remedy against this risk is a sub-division of the power plant into a sufficient number of self-contained units to allow of continued flight after failure of one of the units.

(2) It may, as the result of the pilot worrying and concentrating on other difficulties, lead to an inadvertent stall.

Devices calculated to eliminate this danger have already been dealt with.

(3) It may lead to the machine catching fire in the air through the sudden stoppage and/or through internal fractures in the engine or installation.

Accidents of this kind are happily extremely rare. The chief precaution taken against them in this country is the provision of fireproof bulkheads and of drain pipes for conducting petrol leaks outside the fuselage. In France, however, where fire in the air is rather more frequent, automatically operated fire extinguishers are now standard equipment. It will be remembered that one such extinguisher was demonstrated at Croydon a couple of years ago, as fitted to an old S.E.5.

The safety device provided in this country for use *in extremis* is the parachute, which will be dealt with presently.

### *Structural Failure*

The parachute is the only safety device which is capable of dealing with structural failure in the air. Except in the case of experimental machines, such failures are practically unheard of in British aircraft. A paper on parachutes was recently read before the Society by Flight Lieutenant F. O. Soden, and it is obviously impossible here to add anything useful to what was then said on the subject. However, it may be of interest to note what are the cases of extreme danger from which the parachute provides the possibility of escape. They are :—

Structural failure (including that caused by enemy action).
Collision.
Fire.
Fog.

### *Collision*

Collisions in the air occur chiefly due to the poor field of vision of the pilot in most modern machines. To improve this is a matter of design of the machine as a whole. In this connection, however, one might regard the periscope employed by Colonel Lindberg on his trans-Atlantic flight as a safety device in the true sense of the word.

### *Weather*

When atmospheric conditions are such that map reading is possible, the only aids to navigation required are a map, a compass and a watch. When, however, conditions of visibility are such that real navigational methods have to be employed, then the ability to fly a straight course without reference to the horizon or the ground becomes essential.

In fog or in cloud, even the best compass does not make this possible, and so a turn indicator must be employed. The gyroscopic type of turn indicator has now superseded all others and must be regarded as an indispensable safety device.

### *Reid Turn Indicator*

This instrument embodies a gyroscope which, as is well known, has the property that when an angular displacement is given to its axis of rotation, then that axis tends to rotate in a plane at right angles to the plane of the impressed displacement. In the Reid turn indicator, angular motion of the gyroscopic axis is restrained by a spring, and the position of equilibrium of the gyroscopic axis depends upon the rate of the impressed turn, which is the rate of turn of the aeroplane. The greater the rate of turn, therefore, the bigger the deflection of the gyroscopic axis. The deflection causes lights to appear on the starboard or port side, depending on the direction of the turn, and the greater the number of lights, the higher the rate of turn. The sensitivity of the instrument is adjustable and different pilots obtain best results with different sensitivities. So long as no light appears, the course is, to all intents and purposes, straight. A deviation

to starboard or port immediately causes a light to appear on that side and the pilot can then correct his course appropriately.

### *Weather Dangers*

Failure to find one's destination due to bad visibility involves a number of serious risks. Either one may be tempted to land on unsuitable ground, preferring to do so before the petrol gives out, or the petrol may actually give out whilst the pilot is endeavouring to find his way The Reid turn indicator, combined with ordinary navigational equipment, should enable the pilot to fly a true course, even in conditions of no visibility.

It is perhaps not out of place here to make a plea for a reliable petrol depth gauge as a safety device. Strong efforts have been made in many quarters to produce a suitable instrument, and one must look forward to Major Stewart's forthcoming lecture before the Society, in the course of which he will doubtless bring us up to date on the subject.

### *Wireless Direction Finding*

The fundamental principles of wireless navigation are now well known, but a very brief review may be of use. The direction of incoming wireless waves can be determined by employing a loop aerial which is capable of being rotated about a vertical axis. Such a loop, when rotated, gives a response in the telephones depending on the angle between the plane of the loop and the direction of the incoming waves. If the plane of the loop is parallel to this direction, there is maximum response, and when the plane is at right angles to the direction of incoming waves, there is no response. The most accurate observation of the direction can be made by determining the minimum, and in such a simple system the loop is rotated through the minimum until signals are just audible on either side, these two positions of the loop being recorded and the direction of the incoming waves is that which bisects the angle between them.

Such a simple loop is used practically, and in addition two other systems are used, the Bellini Tosi and the Robinson systems, both of which are developments of the simple loop to give special advantages of a practical nature.

Without going into the details of these practical systems we shall consider how direction finding is actually employed. The actual observations of direction may be made either on the aircraft or on the ground; that is, one or other of these stations, the aircraft or the ground station, acts as a transmitter. The most direct method is for the ground station to transmit, for then the direction is determined on the aircraft and the operation is complete. In the other case, where the aircraft transmits, the ground station determines the direction, but the operation is not complete, for then it must in turn transmit the result to the aircraft. The former direct method has many advantages, particularly in speed, and as regards the fact that for one transmission at the ground station, any number of aircraft can determine their own directions at the same time.

There is, however, as regards aircraft, one very great difficulty in this method, and that is that the determination of direction on an aircraft is not an easy operation. It cannot easily be done by the pilot himself. Again, the metal work of the aircraft distorts the waves, so that errors are introduced which are usually of a quadrantal nature, and these have to be determined for each aircraft. Even then there is another difficulty that these errors sometimes vary with the wave-length.

However, provided there is space and allowance for weight, and again that a special wireless operator is carried, such measurements of direction on an aircraft are of the utmost assistance to navigation. The problem of the single-seater aeroplane is, however, usually outside of these conditions, and no con-

sideration of safety conditions is complete if it does not provide for this case. One system of direction finding on aircraft actually does provide for this case, and that is one system of Robinson, *i.e.*, his wing coil system. In this case the aerial loops are fixed to the wings or to the structure of the aircraft, and the direction of an incoming signal obtained by rotating the whole aircraft. In such a case there are no quadrantal errors. It is granted that this does not form a complete solution of wireless navigation, but it does solve the problem required—that of safety in flying. It enables a pilot to head direct for his objective, and further the operation can be done by the pilot himself. Thus, without much weight or space, and with a simple operation, the danger of a pilot being lost is avoided, for he can always find his direction home, or to the nearest aerodrome.

By such wireless navigation methods a pilot can bring himself to the neighbourhood of an aerodrome, and he can be certain, even in fog, that he is not more than a mile or two away. The question next arises as to whether wireless, which has brought him so near, can be employed to enable him to land just as safely as if fog did not blot out the landscape from his vision. This is a problem for the future and which is by no means improbable of solution. To achieve this result, direction finding might be employed, but will need to be automatic. Another possible line of development is by means of television, although again, though not improbable, no immediate solution is in view.

In the meantime, other electrical principles might be employed to enable a pilot to land on an aerodrome in fog. There are two principles which may be of use in this connection. One of them is the leader system, which consists of a cable through which alternating current is passed. By direct induction a pilot can observe when he is in the neighbourhood of such a cable. It has been proposed to lay cables along air routes, but the expense of such installation, and the fact that on an aeroplane special apparatus would need to be carried in addition to wireless, makes this system fall far behind the practical systems of direction finding for flying towards an aerodrome. However, for actually landing in fog it is conceivable that a satisfactory system can be installed at aerodromes which will enable a pilot to determine at any instant, and automatically, his height and his actual position with regard to such a cable.

The other principle which might be employed is that the electrostatic capacity of an aircraft depends on the distance from the earth. According to well-known formulæ, the electrostatic capacity varies inversely as the distance. This principle up to date has not been very fruitful, for this purpose of landing in fogs, as there are various difficulties, such as the fact that the measurements must be made on the aeroplane itself, and if a special aerial is employed for this purpose, its variation of capacity with height is almost completely masked by the capacity of the aeroplane, unless a very long trailing aerial is employed behind the aeroplane, which is not practical for fog landing.

One point in connection with navigation by wireless which has not perhaps received the emphasis which it deserves is this. If aeroplanes are to fly irrespective of weather and therefore in conditions of no visibility, their safety becomes as dependent on the reliability of the wireless equipment as it is at present dependent upon the reliability of the engine in a single-engined aircraft, and it seems to me, therefore, that duplication of wireless equipment will ultimately become a necessity.

### *Stalling as a Weather Risk*

It has already been mentioned that pre-occupation with other things frequently leads to accidental stalling, and bad visibility, with its consequent worry and lacking sense of orientation, is particularly apt to lead to the conditions which have already been described and the available safeguards against which have been fully dealt with.

### *Conclusion*

It has been somewhat difficult to decide where to make the distinction between, say, an instrument, a device or a feature of design tending towards safety, and this paper will no doubt be justly criticised for the non-inclusion of many elements which, in effect, if not in name, amount to safety devices. Had it, however, been attempted to include everything of the kind, the length of the paper would have become impracticable.

Such matters, for instance, as fire on crash and the innumerable precautions suggested or practised for its elimination have been purposely left out. That subject is extremely important, but so vast and complicated that it deserves separate treatment. Likewise, such matters as petrol systems, engine installation and undercarriage design have been excluded.

In conclusion, allow me to express my gratitude to the Society for having given me this opportunity of addressing them on a subject in which I am personally deeply interested. Further, I should also like to thank those who have assisted me with valuable information and otherwise, notably Mr. Handley Page and Dr. Robinson.

## Discussion

Dr. J. Robinson: In this interesting account of various safety devices for aircraft which we have just listened to, one feature stands out, which is the tendency towards making such devices automatic. The lecturer in one case, that of anti-stalling devices, distinguishes clearly between the different types of methods proposed and rightly states that the best solution is that which is completely automatic, needing no operation on the part of the pilot, that is, in this case the new Handley Page device, about which no further details were given.

With this final objective in view, it is interesting to note certain devices in particular which have just been described. One of them is the Reid turn indicator, which, in addition to being an excellent instrument from the mechanical point of view, employs the very simple process of indications by light. The immediate appeal of a change of colour to a tired pilot who has many operations to perform, cannot be over emphasised, and from this point of view the indications and succeeding operations tend to become automatic.

The subject on which I can speak with most detailed knowledge is wireless, and Mr. Bramson's description of the various methods of wireless navigation are very clear. Direction finding equipment undoubtedly affords a very useful form of safety device for aircraft, and I am certain that many lives which have recently been lost in the attempts to fly the Atlantic would have been saved had such equipment been carried. It is possible to have such equipment installed for comparatively little weight and absorbing little space, and as Mr. Bramson indicates, if the wing coil system is employed, it is very easy to operate, even on single-seater aeroplanes.

Mr. Bramson in describing the wing coil method, where the pilot keeps the aeroplane heading to a wireless station as his objective, shows that drift will make the resultant course curved; but again, he shows that even in these circumstances it is worth while. In fact, the course need not be curved, for it is a comparatively simple matter to correct for drift in such a way that the wing coil method allows a straight course to be flown direct to the objective.

Mr. Handley Page: There is one point I should like to mention with regard to turn indicators. It is, that the standard indicator used by the Royal Air Force is the one in which I happen to be interested. It may be of interest to those present to hear something of the way in which that instrument functions. It is an electrically driven gyroscope as distinct from the Reid, which is air-driven. The advantage of the electrical drive is that, running at a constant number of revolutions, irrespective of the forward speed of the machine, the sensitivity

depends upon the rate of turn, and nothing else; whereas when air-driven, the speed of the gyro will depend on your forward speed, and the sensitivity will depend on the speed at which you are travelling. Most turn indicators suffer from the difficulty of being controlled by springs, or having some frictional resistance in their mechanism, either by the necessity for making contacts or by their damping service. In consequence, the indicator does not act until you are turning at a certain minimum rate.

The Schilossky-Cook gyroscope is gravity-controlled, and therefore there is no frictional device of any kind to retard indication of the movement made, the movement of the indicator being damped by suitable air dashpots. It is mounted on knife edges, which being frictionless, permit a small number of revolutions a minute—1,200 r.p.m.—to give the required sensitivity. It also depends on a visual presentation, but not on moving contacts. It has an ingenious device of two screens—each containing one red and one green sector—which can be seen by the pilot, the light shining through both the screens at once, so that it is dark to the pilot when on a straight course. On a turn two similar colours overlap and give the required indication of the turn.

I have described this device in some detail because it is now being used as a standard instrument in the Royal Air Force, and a great many experiments have been made with it both in fighters and slower speed aircraft.

There is another subject which is also very dear to my heart, and to which Mr. Bramson has very kindly referred, namely, the development of the slot. I should like to say that, with due respect to Mr. Bramson's own gear, my personal feeling is that a great many of these safety devices are unnecessary if the controls of a machine are normal at full speed, in which case stalling should present no danger. The chief problem in stalling is that your lateral stability vanishes, and also you have no control with which to right yourself from a rolling movement in one direction or another. If you can restore to the aircraft its natural stability and lateral control, then the machine has just as good controls as the ordinary controls at full speed. The difficulty, to-day, is that the pilot has to learn two things—firstly, to fly in a certain way if he has not stalled—and secondly, what he must do if he has stalled. The result often is the inevitable spin and loss of height before the pilot can regain flying speed with the nose dive which must precede normal flight.

I should like to thank Mr. Bramson for getting together a most interesting paper on safety devices, and I hope that some of them will materialise to a greater extent in the near future.

Squadron-Leader COLLISHAW referred to an electrical system of fog landings which was tried in America last year.

The speaker also stated that he had tested the Reid turn indicator with success, particularly when flying through clouds.

Major STEWART: I feel that safety in aircraft is often a question of one's proximity to the ground. One feels safe in the air, but the degree of nearness to the ground is a worry.

I should have liked the lecturer to have mentioned that the anti-stall gear comes into action when landing and taking off and that in such cases it may be a disadvantage. He has, of course, made arrangements whereby the gear can be cut out, but perhaps it would have been an advantage if that feature had been brought out more prominently in the paper, as I believe that particular aspect has been the subject of comment from time to time.

There is another point on which I should like to comment, that is, his suggested use of a periscope in conditions of bad visibility. I believe Colonel Lindbergh used a periscope on his aeroplane because he was shut in, but the use of a periscope in the air is one of the most unsatisfactory methods—unless, of course, unavoidable— of finding out what is below you or before you. If,

by the use of a large field lens, a large exit pupil is obtained and thereby the freedom of head movement is considerably increased, gain is to a great extent illusory owing to the want of definition as the eye moves away from the central position, whilst reflection from the glass adds to the difficulties. If, on the other hand, it is necessary for the eye to be carefully located at an eyepiece, clear definition can be obtained, but the view will generally be very limited in other respects.

I do not think I want to add anything to the turn indicator discussion, but there is something to be said, I think, about the kind of turn indicator to be used in fog landings. Mr. Handley Page brought out one important point about the Schilovsky-Cooke indicator, namely, that, being provided with knife edges, it has a rather more rapid response to a turn than anything which is retarded by a frictional band, with the result that a slight turn off the course is more readily indicated. That, I think, on investigation would be found to be a distinct advantage. One can fly through 8,000 feet of clouds safely with a turn indicator, but the problem of coming in to land and striking a definite spot on the aerodrome calls for an instrument which is quick to respond to small movements in azimuth.

There should be no question of an aircraft changing course without the pilot becoming aware of it.

There is one scheme which Mr. Bramson did not bring out in his description of devices for landing in fog, and that is the one proposed many years ago by Professor Lindemann, of putting up two balloons above the clouds. That seems to me to be one of the most promising methods of finding a safe path to an aerodrome and I think it is worthy of mention.

Air Vice-Marshal Sir W. S. BRANCKER: I congratulate Mr. Bramson on his excellent lecture. He said one very satisfactory thing—that British aircraft had probably established a higher standard of safety than those of other nations. About two years ago, as a result of a paper which was read before this Society, I was criticised for attributing too many of our accidents to errors of judgment. I think, however, that I made it clear that I was not blaming the pilots and that these errors of judgment were practically inevitable with the aircraft available; it was up to us to improve the aircraft to a standard in which they should be perfectly safe to fly under any conditions, instead of having a certain inherent danger as they had at present.

With regard to stalling, I think that it is fairly obvious that an apparatus which prevents stalling is not so important as one which maintains control after a stall. For that reason, I think that Mr. Handley Page's invention is far ahead of anything else I have seen. As he has explained, through certain legal difficulties connected with patents, I am not at liberty to give you full details of the construction of the automatic slot at present; I can only say that the device is entirely automatic and needs no attention whatever from the pilot. It does not detract from the speed, it adds practically nothing to the weight, and it adds practically nothing to the cost—except, of course, the royalties which, no doubt, Mr. Handley Page will impose! I was yesterday flying in a machine of an admittedly unstable type fitted with these slots; with his stick pulled back as far as it would possibly go, the pilot maintained perfect control; we merely descended like a parachute without any inclination to spin or nose-dive. It seems to me that the problem of preventing the spinning nose-dive has at last been solved, which is a very big step forward. There is one point to remember, however; for fighting in the air, I doubt if this particular device will be of any value, because a machine which refuses to spin would be at a very serious disadvantage in air combat with a machine which could spin.

Having now eliminated one of our chief dangers—loss of control after stalling—I look upon the next most serious danger of the future as that of collision, particularly in the neighbourhood of an aerodrome in foggy weather. I am all

for television if someone can evolve a means of using it. I always feel that we should be able to discover a ray which is outside the ordinary spectrum, but which would be suitable for penetrating fog as a sort of fog signal. It may be that ordinary wireless can be adapted to do this, and can indicate automatically the position of aircraft moving in the neighbourhood.

The actual landing at an aerodrome during fog is one of our greatest problems in this country. The idea of using two balloons has often been suggested; but it is expensive, and I think it would be only of really practical value in one definite state of weather—that is a fog with a clear sky above it; this condition is actually rare. What we usually have to compete with is bad visibility near the ground with various different cloud strata above it; balloons under these conditions would not only be useless but would be dangerous.

The electric magnet idea is attractive, though it sounds to me that it might considerably increase the number of bad landings.

We are going on with experiments with the leader cable and these are being conducted at Farnborough. We have come to the conclusion that this invention has not yet quite reached the point when we should be justified in expending money on it at Croydon. Mr. Bramson criticised the fact that the leader cable had been used up to date only in large curved circuits. I have already raised this question, but the experts tell me that the scheme will only work with a full closed circuit. Thus, in order to avoid curves near the aerodrome we must have a very long closed circuit, which involves more money spent on the ground.

I do not think that Dr. Robinson did himself justice in omitting to refer to the progress he has made lately in experiments with a beacon which enables aircraft to obtain their direction without carrying any additional apparatus on board. The beacon works with an ordinary wireless receiving set.

Mr. C. G. Grey: It is rather amusing in this year 1927 to find that the aeronautical community has awakened to the existence of the Handley Page slot. It was something like five years [*Note.*—Actually it is 7½ years.—Ed.] since Mr. Handley Page made the fact of the slot public, and I personally regard its advent as a solution of about 90 per cent. of the troubles that beset aviation. It will undoubtedly help towards the making of aeroplanes which an ordinary person can fly with about as much skill as is necessary to drive a car. The only regrettable thing is that it should have taken five years to develop.

Of course, the whole trouble so far has been the weight of the mechanism and the difficulty of producing a mechanism which was not so stiff in working as to upset the pilot's control of the machine, and this automatic operation of the slot is just one of those utterly simple solutions of what appears to be a difficult problem, which puts the whole thing inside the realm of commercial application. It is the same sort of thing as the introduction of detachable pneumatic tyres. I am sure Mr. Handley Page's name will go down to posterity as the saviour of aviation.

Sir Sefton Brancker remarked that he thought that one of the limitations of the slot was that it prevented a machine from spinning. I think he will bear me out in saying that if you want to spin a machine with a slot you can do so by flying it into a spin without stalling it.

I should like to congratulate Mr. Bramson on the excellence of his paper, and we ought to sympathise with him very heartily, because if Mr. Handley Page had not produced his beautiful automatic slot, Mr. Bramson would have been able to make a fortune out of the anti-stall gear. It is like the invention of the Michelin detachable pneumatic tyres before Mr. Welsh's patent arrived. However, we should all have been very sorry if Mr. Handley Page had not arrived, slot or no slot.

Squadron Leader G. H. Reid: I congratulate Mr. Bramson on his excellent paper, and the manner in which he has brought out the many important points.

The aeroplane should firstly, in itself, be aerodynamically safe, and secondly, any shortcomings in this should be tackled by the use of apparatus and instruments. Apparatus and instrument will always add more weight and anything that can be incorporated in the design to save them will, of necessity, give greater efficiency. At present certain apparatus appears necessary, and in connection with the remarks on the Reid control indicator I would like to point out that this instrument was designed for the pilots and from their flying point of view. Tests were carried out and anything thought to be an advantage was tried out and incorporated in the instrument. A case in point was the placing of the air speed indicator in the centre of the instrument, so that all three controls should be indicated from the one instrument. With this arrangement all the test pilots used to spin into clouds deliberately and recover easily whilst inside. A point was raised by Mr. Handley Page about the contact springs. I am afraid Mr. Handley Page misconstrues the technical functioning of the inside of this gyro. The springs are not there merely for contact, but mainly for damping action.

The Reid indicator is not built only for fog flying, but for flying in all weathers such as in clouds at night over hills, where the conditions are usually very trying. The ordinary turn indicating gyro, if adjusted sensitively near the zero, is still sensitive at the outer ends of the scale, and in bad weather conditions this throws a strain on the pilot. To overcome this I fitted four finger springs each side, which reduced the action in bumpy weather, progressively with the amount of the movement and yet retain on the first lamps each side the maximum efficiency for course keeping. This will enable a pilot to keep a straight line course in fog or clouds almost as exact as flying in ordinary clear weather conditions; and this is by virtue of the fact that the first setting of the contact arm can be in the nature of 1/1000 inch. No needle or shutter arrangement can perceptibly indicate this rate of turn, which in this indicator is given clearly by an electric lamp. It is obvious, of course, that the better the pilot the better will he operate on this indicator to keep a straight line course.

The point was raised about the drive. The gyro is driven by a simple and proved satisfactory mechanical drive, namely, a venturi suction on steps cut in the rim of the gyro. This does not depend upon the speed through the air as suggested by one speaker, but depends upon the slipstream from the airscrew. As an example, when the engine is run up the gyro is working and indicates on the ground as the machine takes off. The variation in the rate of the drive of the gyro is practically negligible because the suction power curve of the venturi when the airscrew is going at normal revolutions is practically a horizontal line. When fitted to any particular type of machine there is no difficulty whatsoever from this drive. When transferring an instrument from one machine to another with a more powerful slipstream, this suction can be simply adjusted by altering the setting of the throttle opening. A new model of this instrument is in hand, whereby the weight and size is considerably decreased, as the new air speed indicator is about half the size. The gyro is contained within the indicator and the complete instrument with the three movements are enclosed in an indicator 5in. square. The throttle can be set at any opening for any type of machine and immediately the engine is run up the gyro is running. No accumulator need be carried and a small dry battery is sufficient to run the instrument for several months. The instrument is ready for day and night flying and in addition lights up the air speed indicator.

The instrument is standard on all the machines of the Imperial Air Line and in several countries. The instrument has been used on most of the long-distance flights and has always given the greatest reliability and proved equal to any call made on it. Pilots have flown for several hours on it without any other help and in circumstances that any breakdown of the indicator would have seriously jeopardised the flight. It has proved that a pilot can fly in the worst of weather

without any visibility for several hours and without excessive strain, and the reason for this is the amount of practical considerations put into the design from the suggestions of the best pilots and not merely from technicians with no flying experience.

Gyroscopic work is very useful for many problems in the air, and the fact that the indications from this gyro can be taken to any part of the aeroplane will be of great use in apparatus now being designed for photography, bombing and navigation and will save weight in adding additional gyros, the one instrument fulfilling the various purposes.

One point that has not been brought out this evening, but which must ultimately be of great importance, is the complete automatic control of aircraft. When this has been made reliable, light in weight, and coupled with good aerodynamic efficiency and stability of the aeroplane, we shall have reached as near perfection as can be hoped for and will considerably eliminate the addition of various apparatus and instruments, which until then merely act as a stop-gap.

Mr. E. F. SPANNER (*contributed*): Although not personally engaged in aircraft design or construction, I am intensely interested in the development of heavier-than-air craft for the reason that the safety of this country—in fact of the British Empire—depends upon our having a proper appreciation of their value as our first line of defence.

It is imperative, in my opinion, that the aeroplane should receive far greater attention from those responsible for Imperial defence than is the case at present.

But, while I hold these views as to the paramount importance to us of the aeroplane, I would ask the very serious consideration of this institution for certain practical difficulties yet existing in connection with flying.

I will refer to only one point at the moment.

Do those who look forward, so eagerly and hopefully, to a tremendous expansion in commercial flying realise that there is not yet a safe "rule of the road" for aircraft while in the air?

I know that rules were laid down by the "Convention for the Regulation of Aerial Navigation" in 1919. It is only after a close study of these, and of such other information as I have been able to obtain, that I have arrived at the conclusion that risk of collision in the air is a factor destined to affect tremendously the future of commercial aviation—a factor to which far too little attention has been given to date.

There is very great need that much of the time and attention which is being given, by those in authority, to the "boosting" of civil aviation, should be diverted to consideration of this extremely important matter.

Papers such as Mr. Bramson's are invaluable. Work such as is being done towards reducing the risk of aeroplane mishaps by Mr. Handley Page, Dr. Rohrbach, and others, deserves the fullest possible measure of support from all quarters.

I feel sure that keen, frank criticism must be welcome to the technical members of this Society—even if it comes from one who can only bring forward ideas based upon experience gained in a different sphere of work. Honest criticism is always helpful to a good cause.

From my experience of ship operation, I must say that I do not agree with Mr. Bramson that improvement in the pilot's field of vision will materially reduce risk of collision in the future. It will help certainly—but very little.

The problem of eliminating the collision risk is one which deserves immediate consideration. It has not loomed very large yet because there are very few machines in the air. But that it will be one of the determining factors in definitely limiting commercial aviation I feel assured.

The CHAIRMAN: I very cordially agree with Mr. Bramson in his remarks about the secrecy that is maintained in regard to Royal Air Force accidents. He says it cannot be too strongly emphasised that such a policy is a mistake. I strongly support his remarks.

With regard to French statistics, it would have been very interesting if Mr. Bramson could have given us any information about the actual number of hours flown. Information regarding the number of accidents does not help us to reach a conclusion unless we have all details before us.

Mr. Grey has told us that it has taken us too long a time to discover the virtues of the Handley Page slot. That is certainly true, but I think his remarks apply equally to Mr. Bramson's own invention. I happened to be the first to have the privilege of testing it on an SE.5 somewhere about April, 1925, and it was seen about May, 1925, by the then Air Member for Supply and Research, who expressed a very favourable opinion on it, and decided to place an order. I think it is only comparatively recently that the gear has been actually tested out in the Service. It certainly does seem a long time, and although Mr. Bramson pays generous tribute to the Handley Page slot, there is no doubt whatever that the adoption, as a temporary expedient at least, of Mr. Bramson's gear, would have averted a large number of the fatal accidents that have occurred.

Another point Mr. Bramson very properly stressed is the fact that the turn indicator is going to be indispensable, which ever form it may take. It is really surprising to find, even to-day, that some of the highest authorities are not properly informed on a fundamental issue of this nature, and express doubt as to the need of such an instrument on machines used for long distance records.

## REPLY

I am very glad there has been such a free discussion of the paper, for that gives one the hopeful impression that it may have done some good.

I should like to express my gratitude to Dr. Robinson for the information he gave me during the writing of the paper. He stressed the point that direction-finding wireless should now be regarded as indispensable, and that undoubtedly it would have saved some of the many lives that have been lost recently in Atlantic flights.

Mr. Handley Page gave us some interesting information with regard to the electrically driven turn indicator in which he is interested, and described a continuous indication of rate and direction of turn obtained from that instrument.

There is no question of any particular bias in favour of one instrument or another, but, speaking personally as a pilot, I think that one prefers a continuous indication, and it would be most interesting to have the consensus of opinion of pilots on that point.

Mr. Handley Page mentioned that, provided the controls are normal, you have all the stability you need even when a machine is inclined to stall. I do feel, however, that it ought to be emphasised that controls can never be quite the same when stalling as when not stalling. The very fact of an aeroplane being stalled means that it has reached its maximum lift coefficient. In that case, if it is flying without power, it is sinking so rapidly that when it hits the ground it will crash. If you want to decrease the rate of descent the usual manœuvre is to pull the stick backwards, which increases the rate of descent instead of decreasing it. Therefore, if a pilot wishes to regain normal fore and aft control, he must, whilst sinking rapidly, *push his stick forward*—an abnormal manœuvre which is more urgently necessary the nearer he is to the ground. To be sure that pilots will always do this when stalled it seems essential that they should know when they are in the stalled condition. And now that there is no striking loss of control they may more than ever be unaware of a stall.

That brings me to a point I would like to put before the Society, and on which it would be very valuable to obtain opinions, namely, are there any conceivable conditions in which it is desirable to fly stalled? (I am excepting the take off and deliberate stalling near the ground.) I personally have not been able to find any set of circumstances in which it is desirable. On the contrary, when keeping on a leader cable, for instance, I can imagine many conditions when it is most undesirable, as also after engine failure and in bad weather. If then a pilot accidentally stalls a machine not fitted with the Handley Page slot, he will nose-dive and perhaps commence to spin, and in all probability get killed. If his machine is controllable he will suddenly sink at a rapid rate, and in any case, if he is in circumstances of, say, engine failure, he needs every available foot of altitude for the purpose of manœuvring and landing as comfortably as possible. If instead of that he sinks without any possibility of reducing his rate of descent without first increasing it, I think he is very much worse off, but that is no argument at all against the slot—it is merely an argument in favour of seeing to it that the pilot is well warned before he reaches stalling point. This is not a last moan of protest of an interested party, but a contention put forward in all seriousness that there might be a good case for combining some other device with the Handley Page slot. The very latest gear is completely automatic, and I do not know whether I shall be giving a secret away by saying that some part of it does operate in terms of the angle of incidence, and that therefore it is capable of acting as a stall detector. I leave the matter there and merely put it to the Society, and particularly, I think, to Mr. Handley Page. I share with him complete inability to visualise the form in which television may become helpful, but General Brancker referred to it in different ways, and I suppose there must be some possibility behind it.

Squadron Leader Collishaw referred to a certain magnetic kind of fog landing scheme.

The problem of fog landings has been a pressing one for a long time, and I think that, apart from magnetic devices, there is a possibility of a practical solution by a combination of leader cable, direction-finding wireless and an acoustic arrangement such as a listening post for ascertaining the approach of an aircraft as it comes along the leader cable, when the listener would tell the pilot his whereabouts by means of wireless telephony. It is in this connection, I contend, that a straight leader cable does away with one variable, which is a great advantage to a pilot under conditions where every possible variable should be spared him. The disadvantage of a straight leader cable, to which General Brancker drew attention, is that it must, for electrical reasons, be a closed circuit. I think that a large electrical capacity at one end would get over that difficulty; if it would not then, of course, it would be worth the expenditure to have a long cable.

Major Stewart drew attention to the fact that it is not the air which is the danger but the ground, and then went on to say that an aircraft legitimately stalls on two occasions during a flight—once when the pilot chooses to take off stalled and again in order to make a " three point " landing. I think he inferred that the fact that the anti-stall gear indicates to you at those two moments that you are stalling is a disadvantage. Now, in practice it is not. If desired the gear can be cut out altogether; I have myself flown without a cut-out for months at a time. If you do not cut it out, what happens is that if you take off stalled without being aware of it, the machine tries to put her nose down and her tail up. The moment you do get the tail up sufficiently to unstall, you feel the push on the stick disappear and you know it is perfectly safe to pull up the nose of the machine and climb. It is not in the least disconcerting provided you know that the gear is there. I have even found it a convenience in coming in to land, as when flattening out into horizontal flight it is always a matter of some difficulty to know exactly when to make the last big pull on the stick without

"ballooning." With the anti-stall gear fitted you know that if that push comes on as you are flattening out, all you have to do (and it is an instinctive reaction when so near the ground) is to resist it and pull straight back against it, and there will be no danger of either ballooning or of landing with the tail of the machine too high, though on first consideration it might look like it.

Major Stewart said the periscope was useless as regards bad visibility. Although I may have mentioned "visibility," what I actually meant was "vision." I think Major Stewart will agree with me that it is better in case of need to be able to look through a periscope and see what is dead in front of you than to be deprived of that advantage.

General Brancker referred to Major Stewart's point about balloons in clouds. I was going to make the same point, namely, that balloons may be quite good in fog, but occasions frequently arise when the thickness of the cloud strata above the fog is such that balloons would be a danger and not a help.

General Brancker referred to collisions as being the next great problem. I very much agree with him, and am quite at a loss to say what can be done to overcome this difficulty.

Mr. Grey mentioned the question of spinning and the new Handley Page gear, and suggested that if you fly into a spin the slots do not interfere with it. I must say I do not know, but I have no doubt that Squadron Leader England, if he were here, could give some interesting information, because he has been flying the machine practically every day.

Mr. Grey referred to the fact (if it is a fact) that but for the advent of the new Handley Page slot, the anti-stall gear might have become universally adopted. I do not think that is an aspect of the matter that need really be considered, except by Mr. Handley Page and myself. The only thing that matters is that we are getting over a problem that has cost untold lives and caused an immense amount of prejudice among the public which will take a long time to subside, and which must be got over if all of us who are associated with aviation are to make a reasonable living.

Squadron Leader Reid raised a point in connection with the competing claims of his own and other turn indicators. I think that is perhaps a matter which can best be discussed on some other occasion, but one point he mentioned, namely, that competition is good for trade, gives the impression that this lecture is spurring him on to make an indicator which is just as good as this last one. It was most interesting to hear about his new photographic device, because I happen to remember that he got that idea as the result of a lecture given before the Institution of Aeronautical Engineers, at which he was present. He seems once more to have been successful in tackling a definite new problem.

Squadron Leader Reid mentioned the question of complete automatic control of the aeroplane, such as, for instance, gyroscopic control. I did not include that in my paper, because it is perhaps not quite a safety device, but surely one must have automatic control for large machines.

The French statistics do not include actual hours flown as I could not get hold of the figures.

I am glad to find such an authority as the Chairman in entire agreement with me on the matter of the secrecy with which the cause of accidents is dealt with officially. I do not know what is the correct machinery for bringing pressure to bear on the authorities, but it ought to be set up at once.

# PROCEEDINGS

## SIXTH MEETING, FIRST HALF, 63RD SESSION

A Meeting of the Society was held in the rooms of the Royal Society of Arts, John Street, Adelphi, W.C.2, on Thursday, December 1st. Colonel the Master of Sempill (President of the Society) was in the chair, and a paper was read by Major J. D. Rennie on "The Problem of the Long-Range Flying Boat."

The CHAIRMAN, introducing Major Rennie, said that the last occasion on which the Society had had the pleasure of hearing him was in 1923, when he had read a paper on the design, construction and operation of flying boats. Since that time he had done a great deal of work on the subject. Major Rennie had been one of the advisers at the Technical Department of the R.N.A.S., where he was occupied with the development of aircraft generally. Subsequently he had gone to Felixstowe with the late Commander Porte, with whom he had worked during the war and for some time afterwards. It was no exaggeration to say that the late Commander Porte had impressed upon us the importance of the flying boat, and had, in fact, founded the flying boat movement as we know it to-day; and it was due to his tremendous enthusiasm and early pioneer work that we had been able to advance to the present stage. Major Rennie had designed the largest flying boat in the world, and this machine had been on the recent cruise to the Baltic carrying the Secretary of State for Air.

Major RENNIE, before reading his paper, exhibited a cinematograph film, the first part of which showed the Blackburn "Iris I." giving an exhibition flight at Cromer in the presence of Sir Samuel Hoare, the Secretary of State for Air; the second part showed the first flight of the "Iris II." at Brough; and the third part the departure from Felixstowe of Sir Samuel Hoare on the Baltic cruise in the summer of 1927.

## THE PROBLEM OF THE LONG RANGE FLYING BOAT

BY MAJOR J. D. RENNIE, A.R.T.C., A.M.INST.C.E., F.R.AE.S.

### *Introduction*

During this year several long distance flights have been achieved both over the land and sea. The sensational solo flight across the Atlantic by Colonel Lindbergh is still fresh in our minds. Following his example, numerous other long distance flights have been attempted, some with success and, unfortunately, others coming to grief with loss of life and material. These disasters in an attempt to fly over long stretches of open sea by aeroplanes have focussed attention on the much neglected flying boat, and consequently claims have been put forward as to its obvious suitability for long range overseas flights. As usual, these claims for proposed design of flying boats, generally multi-engined and with passenger accommodation in the wings, and equipped with luxurious smoking saloons, etc., are not based on a sound understanding of the elementary principles involved in such a design and exhibit a blissful ignorance of the experience which has been accumulated up to the present time.

It is the purpose of this paper to attempt to set down, in a simple manner, the results of an investigation of this problem, and to show the factors which must be given careful consideration in the lay-out of a design for a long range flying boat operating under known weather and sea conditions.

As the calculations involved in the preparation of the results are extensive, it is necessary to omit them and present the results, mostly in form of curves. Where thought necessary, a brief explanation of the assumptions made will be given.

The problem divides itself naturally into two parts, the first part dealing with the water performance and the second part with the air performance. Before, however, treating each part separately, I propose to consider broadly, without reference to the design possibility or the practical utility of the aircraft, the weight of fuel and tanks required as the range is increased, and the effect on the main plane load factors as the load is increased above the normal load.

Let us consider the case of a three-engined flying boat of gross weight 29,000 lbs. and fitted with Rolls-Royce " Condor " engines, with load factors of 4 C.P. forward and C.P. aft. The weight, less fuel and tanks, but with pilot, a crew of two, and W/T. and all necessary instruments, may be taken as 18,100 lbs. Fuel and tankage is now added and the range and change in load factors obtained.

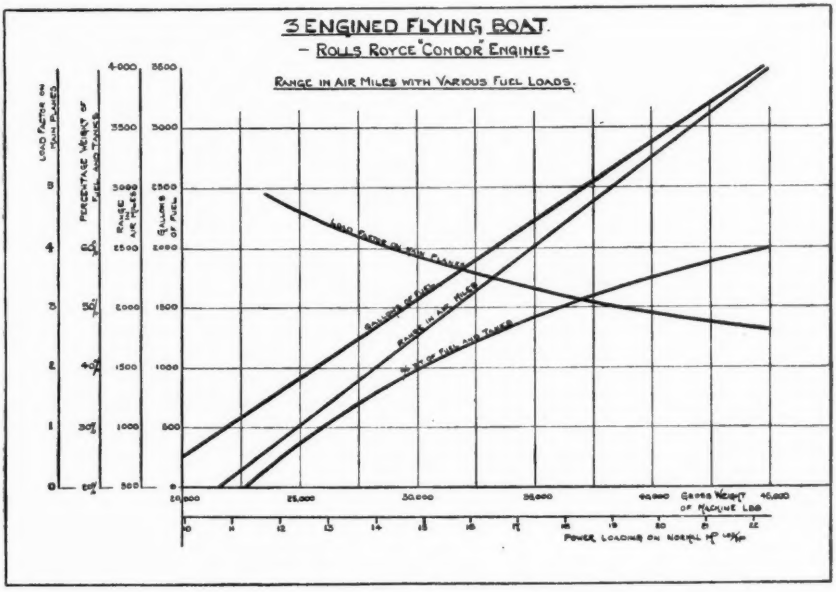

3 ENGINED FLYING BOAT.
– ROLLS ROYCE 'CONDOR' ENGINES –
RANGE IN AIR MILES WITH VARIOUS FUEL LOADS.
LOAD FACTOR ON SPAR BOOMS
PERCENTAGE WEIGHT OF FUEL AND TANKS
RANGE AIR MILES
GALLONS OF FUEL
LOAD FACTOR ON SPAR BOOMS
GALLONS OF FUEL
RANGE IN AIR MILES
% WT OF FUEL AND TANKS
GROSS WEIGHT OF MACHINE LBS
POWER LOADING ON NORMAL HP LBS/HP

FIG. 1.

The results are exhibited in Fig. 1. Applying these results to the two following examples:—A flight from, say, Plymouth to New York and Plymouth to St. John's, Newfoundland, requiring a fuel capacity for roughly 3,500 miles and 2,500 miles respectively, we obtain the figures as shown in Table 1.

TABLE 1.

| Range. | 3,500 miles. | 2,500 miles. |
|---|---|---|
| Gross weight ... ... | 42,000 lbs. | 35,000 lbs. |
| Lbs./h.p. ... ... | 21 | 17·5 |
| Load factor ... ... | 2.8 | 3·3 |

In making any deduction from Fig. 1 it must be remembered that the gross weight does not include any useful load and the results apply to the case of overloading a boat, designed for a normal displacement of 29,000 lbs. and incidentally to the largest flying boat yet flown and a proved success.

Assuming then that the load factors at full load are acceptable, and the structure designed to take the maximum horizontal and vertical accelerations likely to be met with during the take-off, as it is during the take-off under the overloaded condition that damage is likely to occur, these flights are a possibility under suitable sea and weather conditions. If, however, the boat is

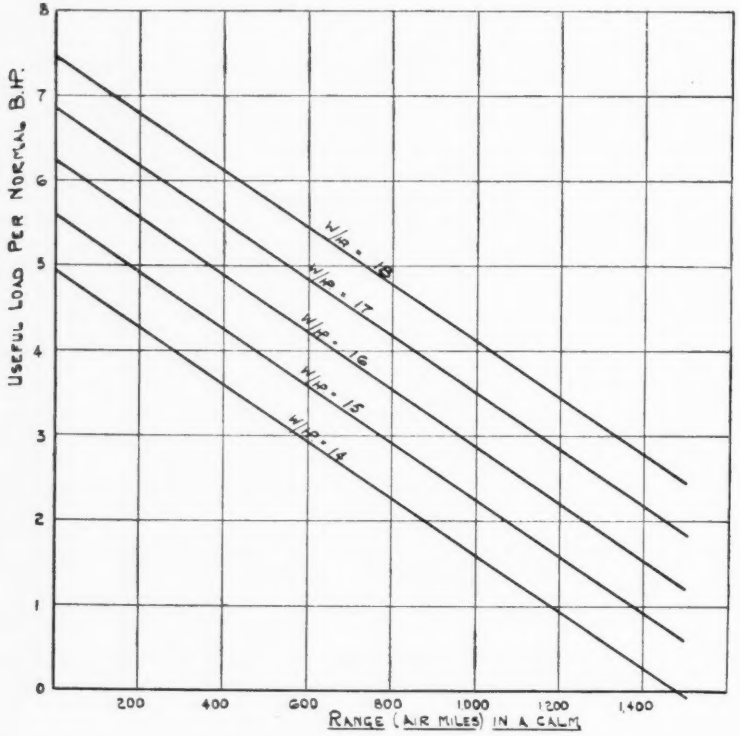



FIG. 2.

designed for these ranges to full factors and to the standard of efficiency obtained at the normal load, such flights are probably impossible at the present time. For example, to retain the same freeboard the hull dimensions would have to be increased about 12 per cent. and 6 per cent. respectively.

The next step to be considered is the relation between the useful or paying load and the range for different horse-power loadings. We will again take the case of a three-engined boat fitted with " Condor " engines and carrying a crew of three and W/T. and instruments, and assuming a structure weight of 37 per cent. calculate the range and useful load for various horse-power loadings. The results are shown in Fig. 2. Obviously, for a given range the paying load increases as the h.p. load increases, but the question to be answered is, " What is the limit to the h.p. loading if operations are to be carried out as required,

under given sea and weather conditions?" However, supposing as the result of experience, 16 lbs./h.p. was taken as this limit, then it should be noted, for a range of 1,000 air miles, a useful load of 3 lbs./h.p. may be carried.

Having now briefly outlined the relation between range, useful load and h.p. loading, without questioning the practicability of any design of boat to meet a definite specification as to range and useful load to be carried, we will now proceed to investigate the various factors involved.

## Water Performance

Under this heading boats may be divided roughly into two classes—fair weather boats and general service boats. The former will generally operate from sheltered bases under favourable sea and weather conditions. A satisfactory design presents little difficulty. Hull forms have been tested, full-scale and model, of low resistance and free from vices such as dynamic instability on the water in its various forms. Under these conditions heavy wing and h.p. loadings are usually employed, leading to a high cruising speed and, with efficient design, to good load carrying capacity.

With the latter type, which may have to operate under adverse sea conditions, seaworthiness is of first importance. While a safe landing is possible in a rough sea, taking-off under the same conditions is an operation requiring great skill on the part of the pilot and involving a certain amount of risk. If a take-off is to be at all possible, then the time and run to get off the water must be reduced to a minimum. A quick take-off demands a low taking-off speed and ample available t.h.p. for rapid acceleration, thus conflicting with the requirements for load carrying and a high cruising speed.

### *(a) Seaworthiness*

Seaworthiness has been the subject of much controversy and has been discussed at some length in papers read before this Society. Generally the problem has been stated under several headings, but I think they may all be grouped together and seaworthiness defined as follows:—A boat is seaworthy if it remains so while on the water, and when in the air is airworthy, and is able to alight in a seaworthy condition and still be airworthy. Apart from structural considerations this means the lay-out of the hull, superstructure and engine positions should be such that either during taxying, taking-off or landing, the comfort of the crew, the satisfactory functioning of the engine and the flight controls is not impaired and the propellers are undamaged. These requirements are easily met under normal load and favourable sea conditions. When, however, sea and weather conditions are unfavourable and/or the boat overloaded, the fair weather boat generally is in trouble.

In this respect, so far as hull form affects seaworthiness, the great majority of modern flying boats are inferior to the " F " boats developed by the late Commander J. C. Porte, R.N., during the late war. This defect is probably the result of lack of knowledge of the experience gained in the development of these and other boats during this period, and to the acceptance of tank test results as providing sufficient data on which to base the design without regard to the full-scale requirements.

Tank tests are an invaluable guide to the prediction of hull resistance and stability under test conditions only and, if used in conjunction with full-scale experience, lead to excellent results in practice.

If experience should show the longest economical range, except for military purposes, to be round about 1,000 miles without refuelling, yet occasions arise when the range has to be extended; this would mean taking-off overloaded, although probably under chosen sea conditions. Then, if the boat under normal

load is seaworthy, it does not follow it will be so overloaded, especially as regards stability, as has been found by actual test. Tank tests should, therefore, be carried out at the overload displacement to ensure, as far as possible, freedom from porpoising.

While it is not within the province of this paper to discuss the design as affected by seaworthiness, as defined above, it may be said if due consideration is given to the type of V bottom on the hull, the form of the fore body and the run of the chine from the main step to the bows, a satisfactory degree of seaworthiness may be obtained, as experience with the Blackburn "Iris" has shown a remarkable advance in this respect without sacrifice in other qualities.

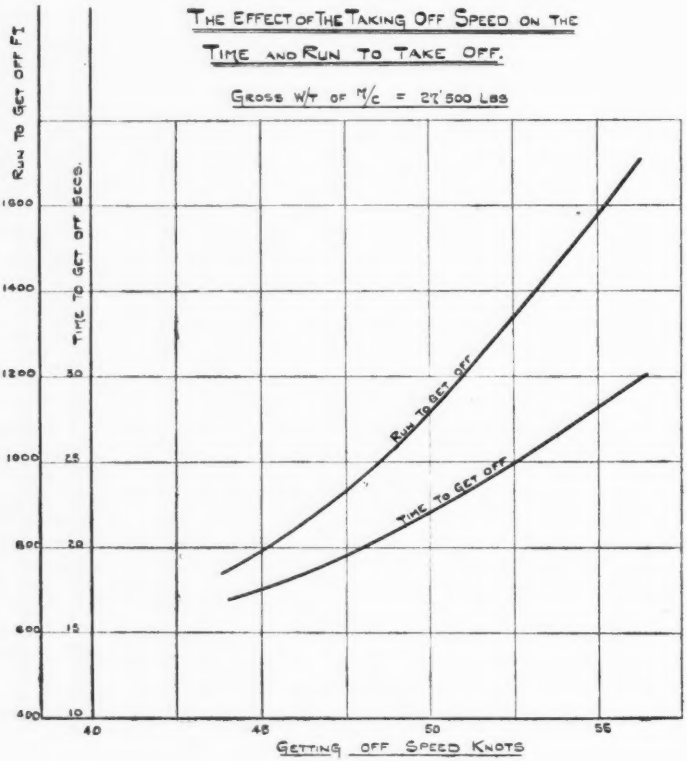



FIG. 3.

It may be accepted, therefore, that a flying boat can be designed suitable to operate under most conditions likely to be met with in which flying may be considered reasonably free from risk.

One other point should just be mentioned. As with a ship, a seaworthy boat may be lost or severely damaged, due to bad seamanship. The indications are that flying boats will be used extensively in the near future; the importance, therefore, of training pilots in the art of seamanship cannot be too strongly emphasised.

### *(b) The Effect of the Taking-Off Speed on the Time and Run to Take-Off*

Calculations have been carried out to investigate the above effect in the case of a three-engined boat of gross weight 27,000 lbs. fitted with Rolls-Royce " Condor " engines. Knowing the water and air resistances, these were adjusted for taking-off speeds of 45 knots, 50 knots and 55 knots, and the total resistance obtained over the speed range up to the taking-off speed. Thus, by integration, the time and run to take-off was found. The main uncertainty in these calculations was the propeller thrust curve, owing to the unreliability of the propeller characteristics at low values of $V/nD$. However, if the results are not strictly accurate they are at least comparative, and probably of sufficient accuracy for our purpose. The results of these calculations are shown in Fig. 3, from which it will be seen, if the taking-off speed is increased from 45 knots to 55 knots—a difference of only 10 knots—the time to take-off is increased by 10 secs. and the length of run by 100 per cent.

Looking at this latter result in another way. In a rough sea the number of waves encountered during the take-off is doubled, thus doubling the risk of damage. This, apart from the serious effect on the engine gearing and the probability of a further increase of run required, is due to the retarding effect of the waves. In the example quoted above, the engines are fitted with a suitable reduction gearing. If ungeared engines had been used the calculations would show, in all cases, a greatly increased run and time to take-off, a result which has been confirmed in actual practice.

It should be observed, assuming these curves to be reasonably accurate, that for the same h.p. loading, the time to take-off is nearly proportional to the square of the taking-off speed and the length of run to the cube of the taking-off speed.

### *(c) The Effect of Horse-Power Loading on the Time and Run to Take-Off at a Constant Taking-Off Speed*

Taking the same example as in (*c*) above calculations were carried out to find the effect of h.p. loading on the time and run to take-off for a taking-off speed of 50 knots. The results are as shown in Table 2 below:—

TABLE 2.

| Engine. | Gross Weight, lbs. | H.P. Loading. lbs./H.P. | Time to take off, secs. | Run to take off, feet. |
|---|---|---|---|---|
| R.R. " Condor " III.A ... | 27,500 | 13.7 | 22 | 1,100 |
| Napier " Lion " X.A ... | 27,500 | 16.1 | 38 | 2,000 |

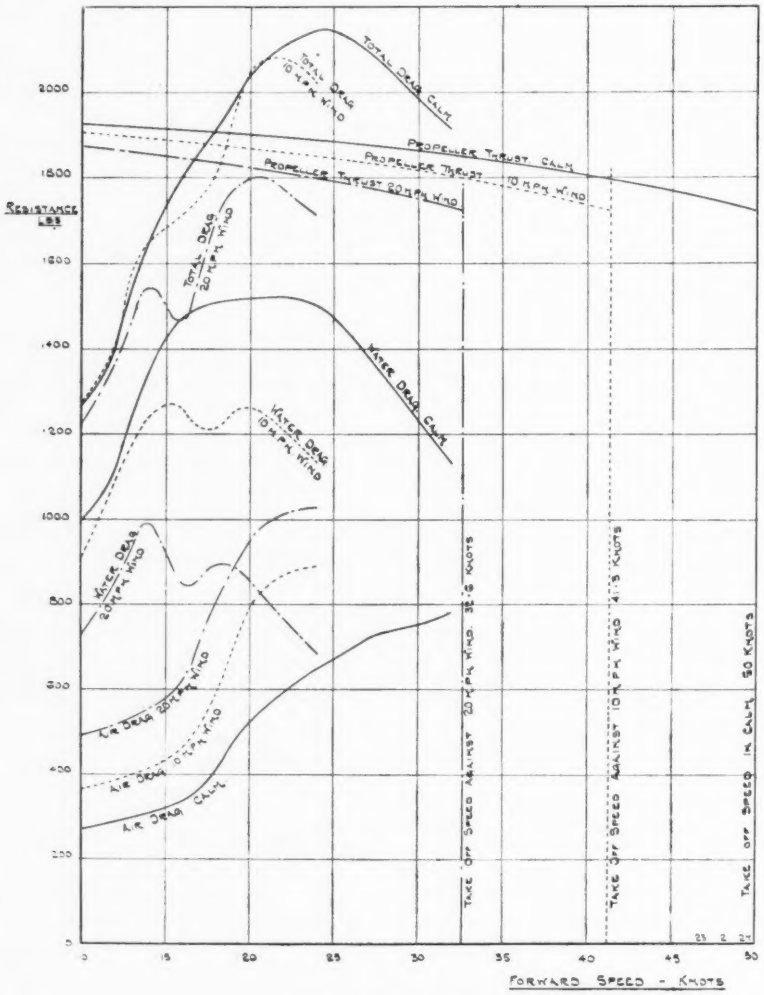
The figures show that except under favourable sea conditions the same remarks apply as under (*c*). The results, however, should be considered in conjunction with Fig. 2.

### *(d) Effect of Wind on the Take-Off of a Heavily Loaded Seaplane*

While it is an accepted fact that a heavily loaded seaplane may take-off in a wind, when it fails to do so in still air, it is of some interest to investigate the effect in the light of tank test data. Fig. 4 shows the resistance, in undisturbed water, of a float system in still air and at wind speeds of 10 m.p.h. and 20 m.p.h. To these are added the air resistance, including slipstream effect, at the appropriate attitude corresponding to the natural running angle of the float, giving the total resistance. The propeller thrust curves are also added. The curves show that the seaplane would not take-off under a head wind of less than 20 m.p.h. provided there was insufficient elevator control to trim to a smaller attitude and so decreasing the air drag.

In practice a reasonable take off was possible at a wind speed less than 10 m.p.h. This may be accounted for partly by the uncertainty in the prediction of the propeller thrust curve and partly to the slightly disturbed water at this wind speed helping to clear the step, so reducing the water resistance.

The chief point of interest is percentage of the air drag to the total drag at speeds round about the " hump " speed, due to the high natural running angle of the float system. In the case of the heavily loaded seaplane, every


RESISTANCE LBS
FORWARD SPEED - KNOTS
TOTAL DRAG CALM
TOTAL DRAG 10 M.P.H. WIND
TOTAL DRAG 20 M.P.H. WIND
PROPELLER THRUST CALM
PROPELLER THRUST 10 M.P.H. WIND
PROPELLER THRUST 20 M.P.H. WIND
WATER DRAG CALM
WATER DRAG 10 M.P.H. WIND
WATER DRAG 20 M.P.H. WIND
AIR DRAG 20 M.P.H. WIND
AIR DRAG 10 M.P.H. WIND
AIR DRAG CALM
TAKE OFF SPEED AGAINST 20 M.P.H. WIND. 32.6 KNOTS
TAKE OFF SPEED AGAINST 10 M.P.H. WIND 41.5 KNOTS
TAKE OFF SPEED IN CALM. 50 KNOTS

TWIN FLOAT SEAPLANE

WEIGHT = 7500 LBS

FIG. 4.

endeavour should be made to reduce this angle to a minimum consistent with the other float requirements, such as the ability to trim back to take-off.

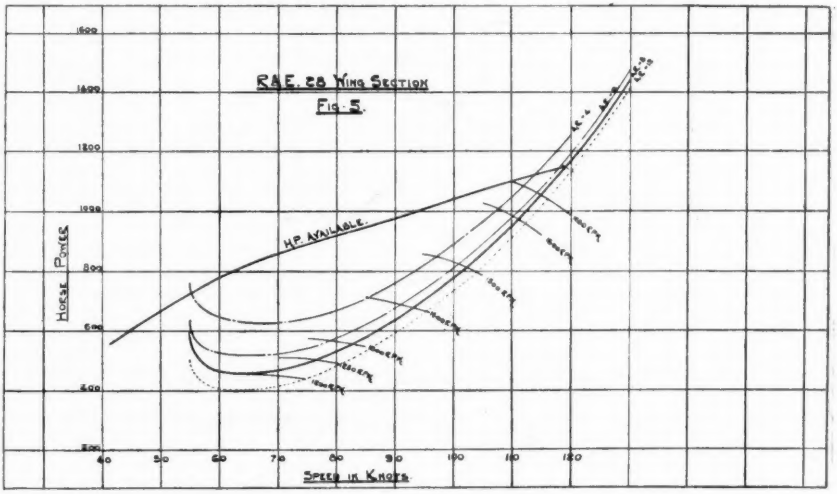
## Air Performance

The ever increasing demand for longer range has been followed up, quite naturally, by an equally insistent need for high cruising speed, in order to reduce the duration of flight for a given range. It has been stated, extensively, in various quarters, this may be attained solely by the process of cleaning up a design. It is of some interest, therefore, to investigate the relative importance of the design characteristics on which the cruising speed and range may depend.

In what follows, unless otherwise stated, the cruising speed is defined as the most economical speed, that is, the speed at which the air miles per gallon are a maximum; this is easily shown to occur when

$$(nL/D)/p \times 375/W \times f(\rho)/\rho \text{ is a maximum.}$$

Where $n$ = the propeller efficiency at cruising speed.
$L/D$ = the lift/drag ratio of the aircraft at this speed.
$W$ = the weight of the aircraft.
$f(\rho)$ = the b.h.p. altitude correction factor.
$\rho$ = the density at altitude.
$p$ = the rate of fuel consumption at sea level.




### *(a) The Effect of Aspect Ratios on the Range and Cruising Speed*

Let us consider the case of a three-engined biplane flying boat, the particulars of which are as follows, and calculate the air performance, range and cruising speed for various aspect ratios:—

| | | |
|---|---|---|
| Gross weight | ... | 17,000 lbs. |
| Engines ... | ... | Air-cooled, giving 450 b.h.p. at 1,700 normal revs. |
| Fuel capacity | ... | 500 gallons. |
| Wing section | ... | R.A.F. 28. |
| Stalling speed | ... | 55 knots. |

The results of the calculations are shown in Figs. 5, 6, 7. In Fig. 6 is plotted the range against the cruising speed, from which it will be seen that as

the aspect ratio increases the range increases, but the cruising speed decreases. In Fig. 7 is plotted the $L/D$ ratio of the aircraft against speed. From these curves it will be seen that an aspect ratio may be chosen to make the cruising speed coincide with the top speed, but owing to the inefficiency of this wing arrangement, there is a considerable loss in air performance, as Figs. 5, 6 clearly show.

### *(b) The Effect of the Taking-Off Speed on the Range and Cruising Speed*

In the investigations to follow, the results are based on the three-engined boat taken as the example dealt with under " Water Performance "; but at a gross weight of 27,500 lbs. and with a fuel capacity of 900 gallons.

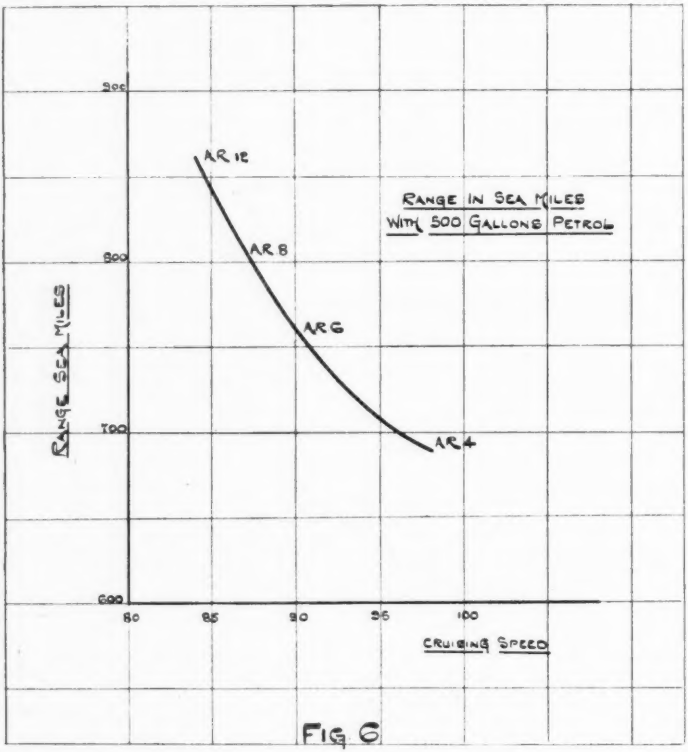



FIG 6

Under this heading calculations have been carried out to find the range and cruising speed at taking-off speeds of 45 knots, 50 knots and 55 knots, using the throttled consumption curves supplied by Messrs. Rolls-Royce for the " Condor " III. high compression engine.

The results are exhibited in Figs. 8, 9, 10, 11. Fig. 8 shows the range plotted against the throttled level speed for the above taking-off speeds, the maximum point in each case giving the cruising speed.

The value of $n = Vc/Vs$, the ratio of cruising speed/stalling speed are found to be 1.4, 1.32 and 1.32 respectively.

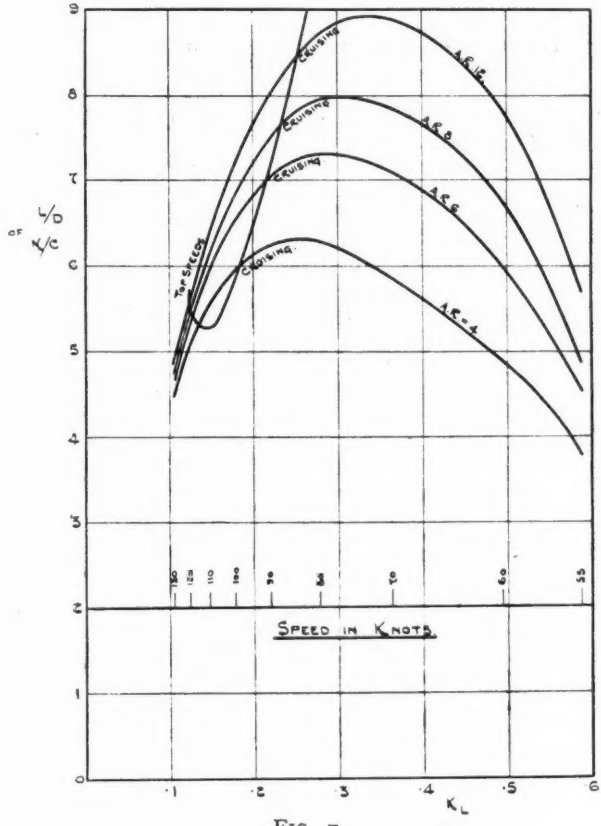

L/D
or
K/C
CRUISING
CRUISING
CRUISING
CRUISING
TOP SPEED
AR 10
AR 8
AR 6
AR 4
SPEED IN KNOTS
$K_L$

FIG. 7.

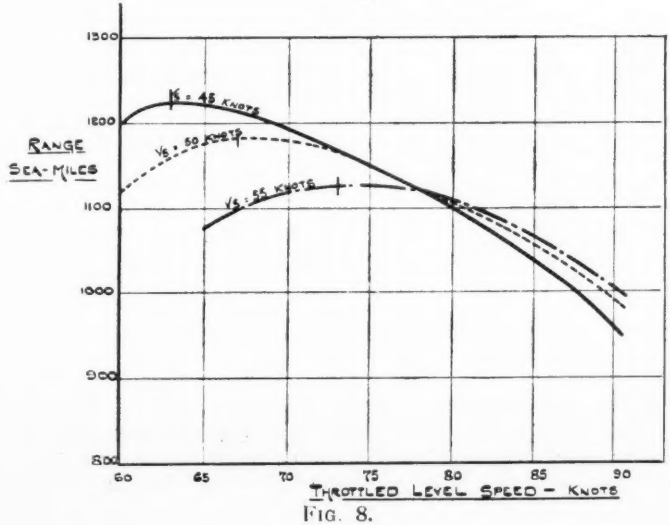

RANGE
SEA-MILES
$V_S$ = 45 KNOTS
$V_S$ = 50 KNOTS
$V_S$ = 55 KNOTS
THROTTLED LEVEL SPEED – KNOTS

FIG. 8.

From the curves it will be seen that as the taking-off speed is increased from 45 knots to 55 knots, a difference of 10 knots, the cruising speed is increased by about the same amount, but the range at the higher speed is decreased about 8 per cent. On the other hand, as Fig. 9 shows, the engine revolutions have gone up from 1,500 r.p.m. to 1,670 r.p.m.

These results are shown more clearly in Fig. 10 in which the cruising speed and revolutions are plotted against the taking-off speed. As an average value of "$n$" of 1.4 is commonly used, the curve using this value has been added for comparison with the results obtained by calculation.

Referring again to Fig. 8, it will be observed the curves are fairly flat. If it is assumed these curves are obtained, even approximately in practice, then, to reduce the duration of flight, obviously it will pay to fly at a throttled speed considerably higher than the cruising speed, provided the engine can maintain the required higher revolutions for long periods.

For example, take the case for 55 knots taking-off speed. The cruising speed could be increased from 73 knots to 80 knots, or 85 knots, with little loss

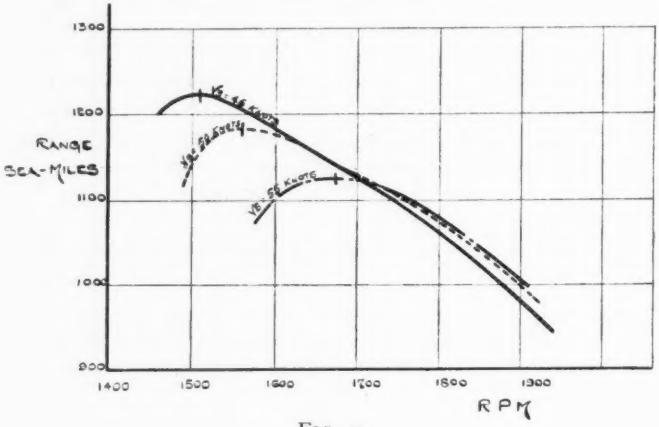



FIG. 9.

in range, if the engines could maintain revolutions of 1,750 r.p.m. and 1,900 r.p.m. respectively.

In Fig. 11 is plotted against the speed the aerodynamic efficiency, $nL/D$ for throttled level flight. If these curves be studied in conjunction with those of Fig. 8, the effect of the aerodynamics on the cruising speed in relation to the taking-off speed will be followed easily.

However, one point of interest may be mentioned as a result of a study of these curves. It will be observed that at a speed of about 80 knots the $nL/D$ curves and the range curves cross. This means that for a range of 1,100 miles, and with the same fuel capacity, the throttled speed, and hence duration, is the same at the same engine revolutions, irrespective of the taking-off speed.

### *(c) The Influence on the Range and Cruising Speed of an Increase in Top Speed due to Cleaning Up*

The calculations were continued for the case of a taking-off speed of 50 knots, but assuming the body drag has been reduced by 50 per cent., a figure unlikely to be realised, thus setting an upper limit to the possibilities in this direction.

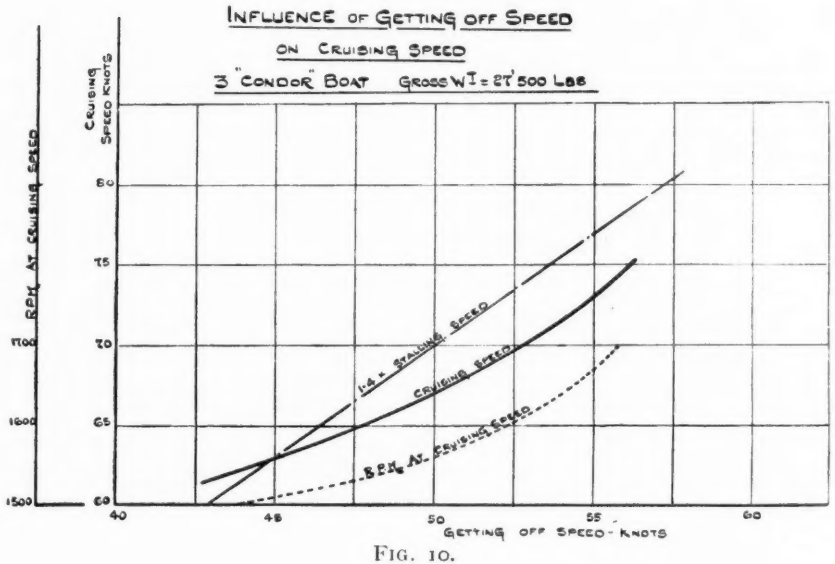



FIG. 10.

The performance calculations show the top speed is increased from 100 knots to 119 knots, and in Fig. 12 is plotted the range and revolutions against speed for both cases. It will be seen the cruising speed is now 75 knots compared with 67 knots, or the gain in cruising speed is less than half the gain in top speed.

The value of "$n$" has now risen from 1.32 to 1.5.

While the gain in cruising speed is small, it will be seen to be accompanied by a slight reduction in engine revolutions (see Fig. 13).

As would be expected, the range has been increased considerably to the extent of about 30 per cent.

### *(d) Range as Effected by Improved Engine Throttled Consumption*

So far, we have considered range and cruising speed in relation to the water and air performance, treating the engine simply as a means of providing thrust, without reference to its efficiency or suitability.

The efficiency, so far as range and cruising speed is concerned, may be measured in terms of fuel consumption throttled and the ability to run for long periods at, at least, 90 per cent. of the normal revs. The importance of the latter, on the cruising speed, was seen clearly from Fig. 8.

With regard to fuel consumption throttled, and to show how the engine constructors can, in a great measure, help in the development of long range flight, either by the adoption of higher compression ratios, by improvements in carburetters, or the use of mixture control, preferably automatic, or a combination of the above. Figs. 14 and 15 have been prepared.

Fig. 14, curve *a*, shows the range plotted against speed, using the generalised curve of throttled consumption, without mixture control, given in N.A.C.A. Report 234 and reproduced in Fig. 16, which represents average values of consumption obtained in practice.

Curve *b* shows the range, using the throttled consumption curves, as supplied by Messrs. Rolls-Royce, for the high compression " Condor " engine. Consumption tests on actual aircraft have given figures in good agreement with these curves. It will be seen from the curves that, as would be expected, there is little change in the cruising speed; but the range, for the same fuel capacity, has been increased by about 20 per cent. Assuming a result of this order to be attainable, and there is every indication that it is possible, then the increase in

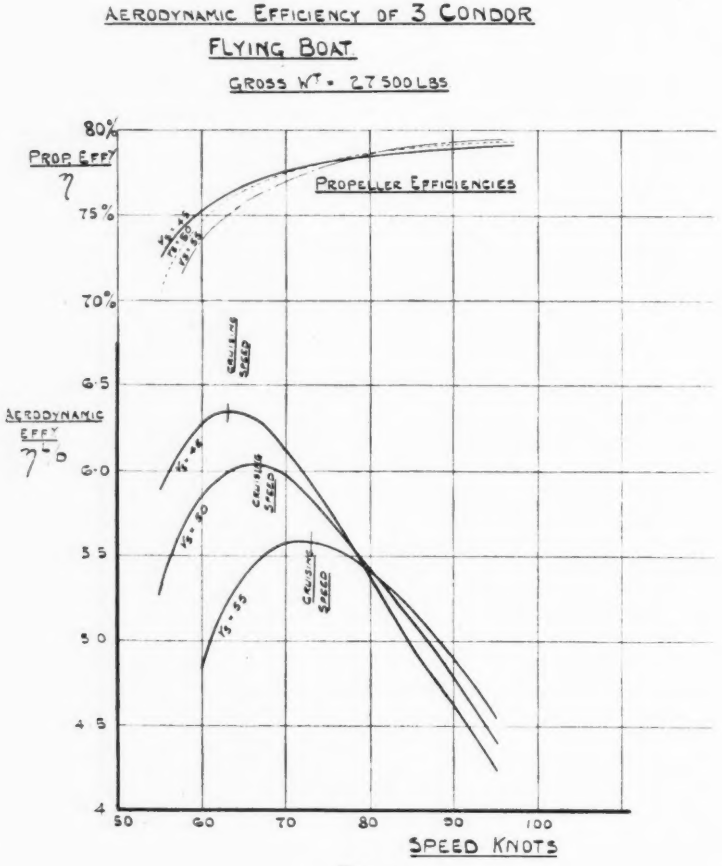


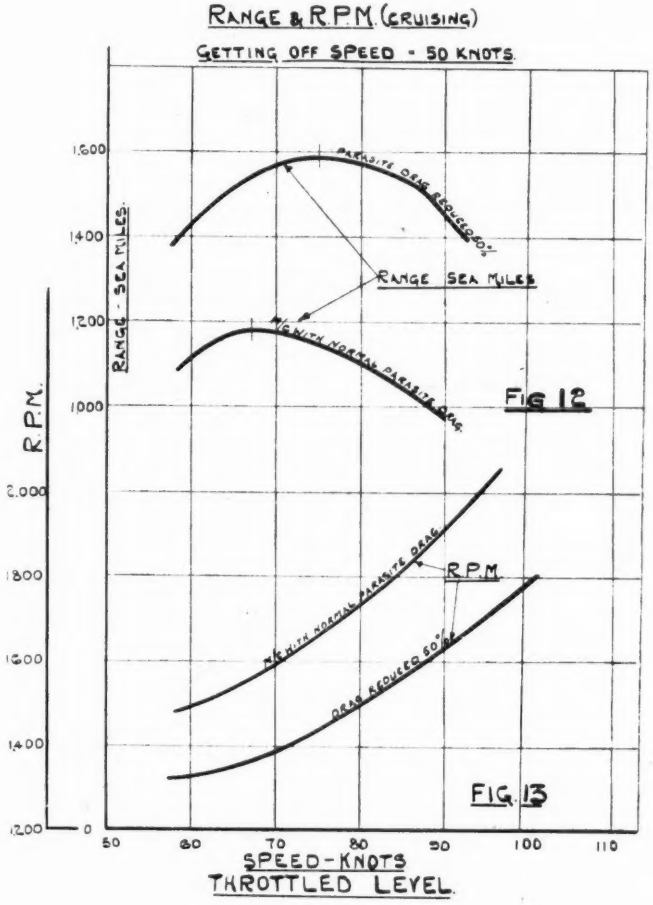

FIG. 11.

range to be expected is of a magnitude equal to that likely to be obtained by aerodynamic means.

Further, as a matter of interest, Fig. 15 has been prepared to show the increase in range due to a reduction in consumption, without considering the theoretical or practical possibility. The curves have been calculated on the assumption that the rate of consumption is constant over the engine revolutions corresponding to the throttled speeds, and the maximum range as found, plotted against rate of consumption.

It will be seen, to take one example, if the throttled consumption could be reduced from, say, 0.5 pts./b.h.p./hr. to 0.45 pts./b.h.p./hr., the range could be increased by nearly 20 per cent. for the same fuel capacity, *i.e.*, 900 gallons.

From the above, the importance of the gain in range, due to improved fuel consumption, cannot be too strongly emphasised, and I feel sure a statement




from the engine side, as to the possibilities in this direction, would be greatly welcomed.

In connection with engine revolutions, it is generally specified that the maximum permissible revolutions must not be maintained for more than five minutes. On the other hand, little or no information is available as to the service to be expected from an engine running, for lengthy periods, at from 90 per cent. normal revs. to normal revs. As has been shown, high engine revolutions, round about 90 per cent. of the normal, are essential for high cruising speed, not necessarily the most economical cruising speed, and as an engine which has

to be nursed at low revs. is useless for long range flights, it would be helpful if the engine constructors would give some definite information on this question for the guidance of those concerned.

Having now considered, in some detail and with sufficient accuracy, the main features in design with regard to the problem of long range, it remains to

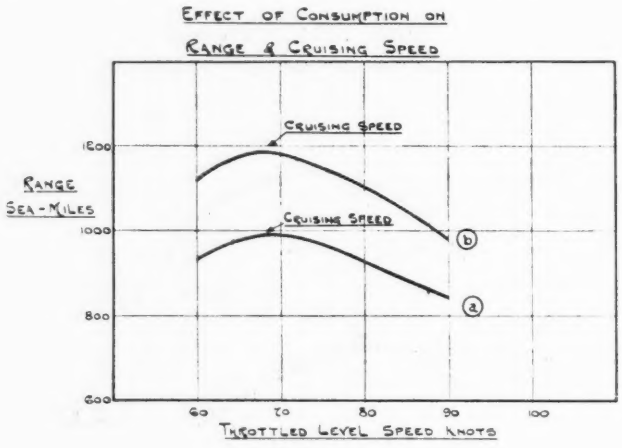



Fig. 14.

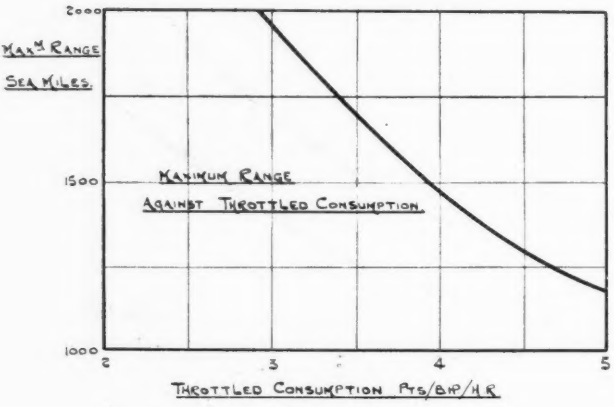



Fig. 15.

discuss briefly the influence of propeller design in this respect and to remark on the subject of flight controls for either the large or the overloaded flying boat.

With regard to the former, it may be said that, while the variable pitch propeller must lead to improved performance—especially as regards take-off in the heavily loaded state—at present it is only in the experimental stage of development, and until the prospects of producing a reliable and simple propeller are distinctly more hopeful than at present, the orthodox fixed pitch propeller will continue to be used. In this case the design may proceed in two ways. Firstly,

it may be designed to develop maximum permissible revolutions at a given speed in excess of the speed specified for normal revolutions. The engine will then run throttled at the specified top speed at normal revolutions. Secondly, it may be designed for top speed conditions at normal revolutions. The first design leads to a lower top speed at normal revolutions than the second; but there is more thrust horse-power available for climb and take-off and, as calculations show, little effect on cruising speed, but a slight gain in range. Unless, therefore, there is difficulty in obtaining the specified top speed at normal revolutions, the first design conditions give the better all round performance.

Considering the latter, it will be obvious that on flights of long duration, the aircraft must not be tiring to fly, especially under bad weather conditions.

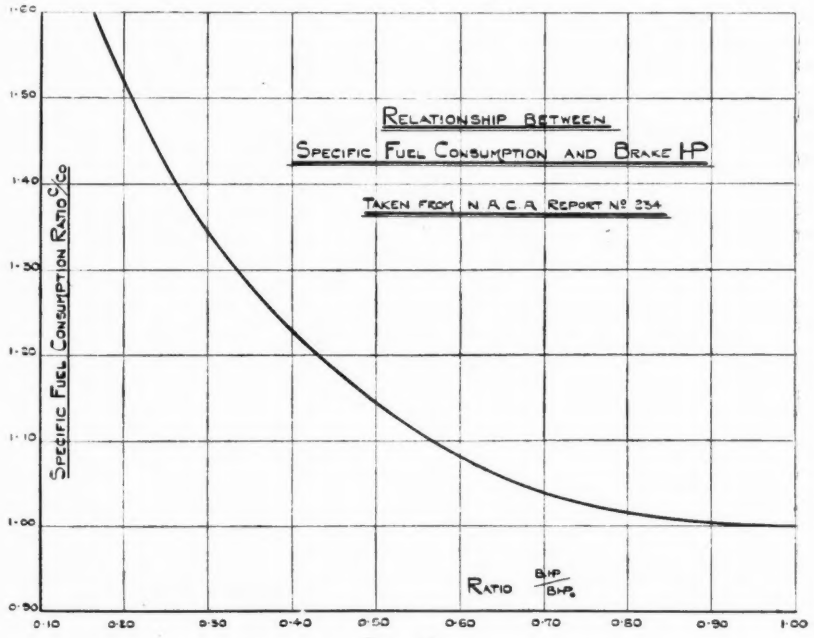



FIG. 16.

In this respect experience has shown that if advantage is taken of the vast amount of research work which has been carried out in this country alone, the problem of the provision of ease of and adequate control under all conditions of flight should present little difficulty either in the overloaded boat or in a boat of a size likely to be contemplated in the near future.

While it is not within the scope of this paper to discuss the many practical problems, mostly unforeseen, likely to arise in the operation of long range flying boats, there is one problem of considerable practical interest, namely, fuelling, on which a few remarks may be opportune.

When it is realised that between flights fuel, to the extent of 1,000 gallons or more, may have to be taken on board, at a rate of not less than 500 gallons per hour, depending on circumstances, possibly under difficult sea conditions and at a minimum labour cost, the problem will be seen to be far from simple.

The operation of fuelling may be considered to take place in two stages. The first stage consists in getting the fuel to the boat, and the second stage from the boat to the tanks. At a sheltered base, the boat may be brought alongside a specially designed jetty and fueled from a laid-on supply pipe. However, such conditions will generally be the exception and fuelling will have to be carried out at moorings. The fuel then will be pumped on board from a fuelling boat, moored well clear and preferably astern of the boat. This operation may appear fairly simple, but when one considers the possible relative motions of the two boats, connected by a flexible filling pipe, it will be realised considerable skill and experience is required safely to carry out the operation.

If it is accepted, the pure gravity fuel feed system represents the best practice in flying boat design. Fig. 17 shows diagrammatically a neat and satisfactory petrol and fuelling system applicable to a biplane boat with one or more gravity tanks. The petrol flows from the tank by means of large bore piping (*a*) to a common distributor box fixed to the hull deck. By means of cocks controlled from inside the hull, the petrol may be fed to the engines via pipes (*g*) from any tank or combination of tanks. Also the tanks may be trimmed as required by the same control.

Fuelling may be carried out in three ways. Firstly, by coupling the filling hose directly to the connection (*b*) on the tank sump. When fuelling has to be carried out rapidly, and there are several tanks, this position allows simultaneous filling of the tanks from separate supply pipes. Secondly, by means of the connection (*d*) and the screw-down valve (*f*) opening into the distributor box, the tanks may be filled separately or together, via the existing pipes (*a*) to the tanks. This system has the advantage that all tanks may be filled at one accessible connection, and it provides a permanent fuelling arrangement without additional piping. It would be used as an alternative, in preference to the above system, when fuelling was not a matter of great urgency. Thirdly, to provide for emergency, the tanks may be filled from tins at the filler cap (*e*) on the tank top.

It will be seen the above petrol feed and fuelling system combines with simplicity the desirable, one may say, essential feature, that the interior of hull is free from petrol or petrol fumes.

## The Multi-Engined Flying Boat

On several occasions, to my knowledge, it has been stated publicly that given engine reliability, and hence immunity from a forced landing, the aeroplane would, owing to its superior aerodynamic performance, become a serious competitor if not entirely oust the flying boat for overseas flights. I venture to say, as a study of the daily Press alone would confirm, absolute reliability in any form of transport, even in the oldest established, does not exist; and it is probably true to say that of all means of transport, the consequences arising from even a minor breakdown in power plant or structure are likely to lead to a more serious loss in life and material in the case of air than in the more accepted forms, such as ship, rail or road transport.

On the contrary, the large flying boat is equal if not superior to the corresponding aeroplane as regards performance and, on the question of safety, I feel sure if two air lines, one operating aeroplanes, the other flying boats, over the same open sea route, the proportion of passengers carried by the latter would soon show the feelings of the travelling public in this respect.

Turning now to the question of the multi versus the twin or single-engined flying boat. It is obvious, for the same power and standard of design, the fewer the number of engines the higher the aerodynamic efficiency. On the other hand, the loss is probably small and may be acceptable in comparison with the

gain in other directions, due to the use of three or more engines. As to the initial expenditure, cost of running and maintenance, so far as I am aware, there is no evidence to show what the difference is, if any, between two or three engines developing the same aggregate horse-power.

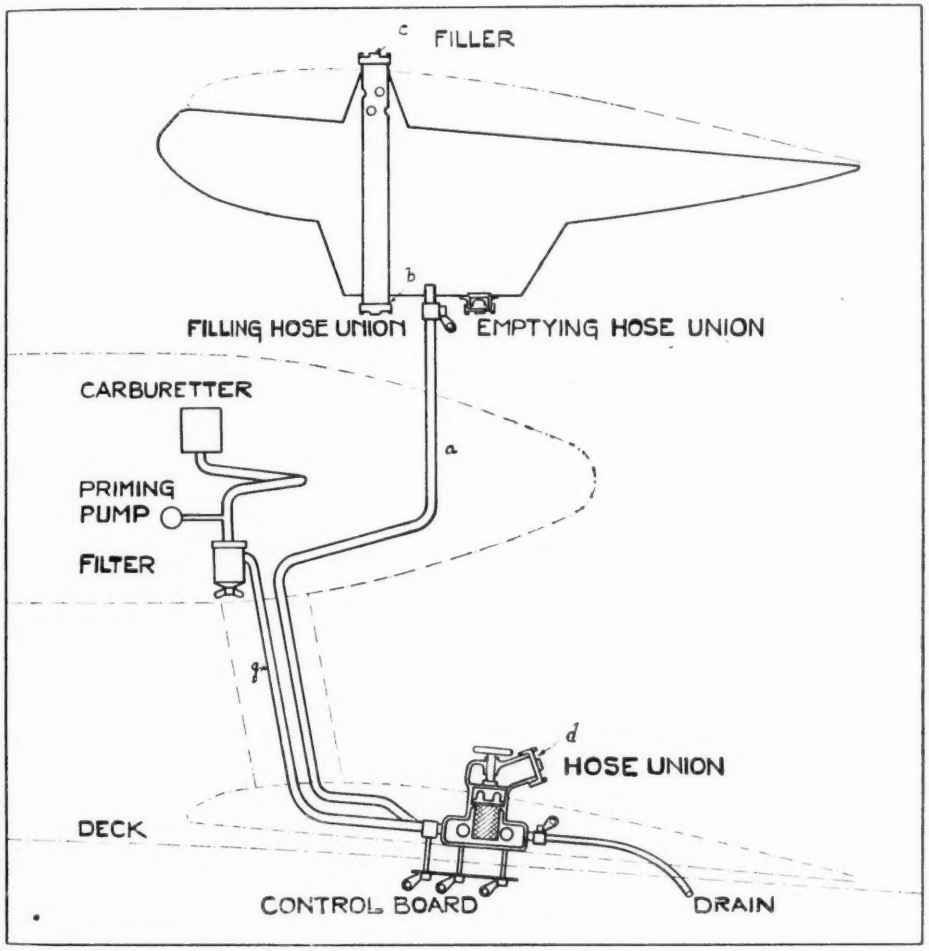



FIG. 17.

In the case of a twin-engined flying boat, failure of one engine means the loss of half, as against one-third of the available horse-power in the three-engined boat. For example, supposing fully loaded the h.p. loading was

15 lbs./h.p., with the former this would mean flight would have to be possible at normal engine revs. with a h.p. loading of 30 lbs./h.p. against 22.5 lbs./h.p. for the latter. In the event of engine failure, the former would be forced to land —generally in the open sea, as there would be little, if any, chance in reaching a sheltered anchorage—and be unable to take-off again unless repairs could be carried out. The latter, however, could carry on in level flight at full load and normal engine revolutions, on glide as calculations show, from a height of 5,000 feet to 500 feet in a distance of about one hundred and fifty miles on two engines running at 90 per cent. full power and revolutions. Under these conditions the desired destination could be reached with little delay, or if so desired and available, a sheltered anchorage could be chosen to carry out repairs before proceeding.

Further, if the engine repairs could only be carried out at a base and it was necessary to make an intermediate stop, the question of the possibility of taking-off again need not arise, as experience has proved, given reasonable sea conditions, the boat will take off with any two engines, even fully loaded, corresponding to a h.p. loading of 21.5 lbs./h.p.

Without labouring the point, the advantages from both the military and commercial aspect of the long range three-engined boat are worthy of serious consideration.

The results of this investigation represent, I believe, fairly completely, the present state of our knowledge in respect to the problem of the long range flying boat. I have no wish to attempt to predict the developments likely to be realised in the future, as many years' experience are still required in the operation of flying boats before the relative importance of the factors which have just been discussed can be definitely stated, and an upper limit set to the possibilities of the flying boat operating under diverse sea and atmospheric conditions to meet specified military or commercial requirements. As has been shown, a quick take-off demands a low taking-off speed and a light h.p. loading, antagonistic to the requirements for high cruising speed and load carrying.

Therefore, other things being equal, the success or otherwise of any proposed flying boat line will depend almost entirely on the correct choice of the taking-off speed and h.p. loading suitable for the known conditions.

With the present knowledge and experience available, appropriate values can be given within fairly narrow limits which, if not the best, will at least guard against failure in the design.

Whereas the flying boat will provide in the near future a means of transport further to speed up many routes accessible only by water, it should be realised every form of transport has its limitation; but fortunately, in the scheme of things, also a definite usefulness in the commercial life and, in no small measure, the prosperity of a nation. As it is most unlikely the flying boat will compete on the main steamship lines, the great shipping concerns, unique to Britain, who have built up a mercantile marine second to none, should be the first to realise the potentiality of the flying boat as providing not only a high speed auxiliary to the main fleet but, to mention only a few possibilities, as a means of opening up communications with suitable interior waters inaccessible by rail or ship, so speeding up the development of new country; also as a means of reaching quickly up-river ports, outlying islands or coastal ports only accessible by roundabout routes.

Trade follows the flag; but what is also of very great importance, trade follows better and quicker facilities of transport. The combined transport service, the flying boat linked up with the steamship, can only lead to mutual benefit.

In their own interests alone, it is up to the shipping companies to encourage the development of the flying boat. It is only a matter of time. The problem

has to be tackled, the sooner the better, and should not be left to foreign competitors to take the initiative and so not only cause loss of trade to ourselves as a nation, but deprive the aircraft industry of work it is in a position to undertake with success now. As with ships so with flying boats, we have always led the way. As a matter of national importance, therefore, we look to those concerned, who by their influence and the resources at their disposal are in a position to ensure that this lead shall not be lost, through either lack of enterprise or ignorance of the possibilities of the flying boat.

In conclusion, I have to thank Mr. Blackburn for his kind permission to present this paper, and Mr. Fenton, of our design staff, for his able assistance in carrying out the calculations involved.

## Discussion

The Chairman said that those present who were not already convinced of the potentialities of the flying boat—and he did not think there were very many—must have had their hearts melted by Major Rennie's very able summary of the position and forecast of its possibilities. Major Rennie's plea on behalf of the much-neglected flying boat was one which he (the Chairman) heartily endorsed; there was no doubt that the craft had been very greatly neglected, although, as had been pointed out, we do lead technically. But we had to do more than that. It was due to the efforts of people such as Major Rennie that to-day's position had been reached, and but for the work of such enthusiasts the position would have been much worse. Major Rennie had always believed in the development of the flying boat, and had continued pegging away at it. There were others present, as, for instance, Major Penny, who had done the same. Personally, he (the Chairman) believed that if there was a substantial future for heavier-than-air craft on Imperial air routes, that future rested primarily with the flying boat in so far as our own needs were concerned.

Major Buchanan, after expressing his appreciation of the paper, regretted that he was not in a position to offer any criticism of the technical results given, because the basis on which they were obtained had not been indicated. With regard to the assertion, often repeated at meetings of this Society, that the flying boat was as efficient as the aeroplane of corresponding size, he said that probably he had access to more records of aircraft performances than had anyone else in the country, and he had never been able to find evidence in support of it. The plain fact, as shown by our records and our experience, was that the flying boat in its present size was heavier structurally than the aeroplane of corresponding size. It was also less efficient aerodynamically. At the same time, it was quite clear that the flying boat was slowly but surely overhauling the aeroplane, and the time was approaching when the flying boat would be at least on a level with it. That time, technically, had not yet arrived. He doubted if many would agree with Major Rennie's statement that very few of the modern flying boats were as seaworthy as the old "F" boats built by the late Commander Porte. Indeed, the boats designed by Major Rennie never looked at all like "F" boats; he had designed them quite differently. Discussing further the relative merits of the flying boat and the aeroplane, Major Buchanan said there was no doubt that those who were keen on making long solo flights such as those made across the Atlantic were driven to use the aeroplane, because it was so very much cheaper; at the moment the flying boat was very much more costly than the corresponding aeroplane from the maintenance and the efficiency points of view. He hoped it would not be thought, in view of his remarks, that he was antagonistic towards the flying boat; on the contrary, for many years he had been one of its strongest supporters, but he felt that no good purpose was served by over-stating the case. The time

was approaching when the flying boat would be superior to the aeroplane, and it was clear that the future of large aircraft lay essentially with the flying boat. He was indebted to Major Rennie for his analysis of the situation, because he had put forward the long-range problem in a new light. With regard to petrol consumption, he pointed out that it was easy to make a set of curves based on rather an insecure foundation. The figures given in the paper, he believed, were based on a high-compression Rolls-Royce engine, but he was not sure what compression ratio that represented.

Major RENNIE: 6—1.

Major BUCHANAN said he had asked because there were other high-compression Rolls-Royce engines which had given good consumption, but which, because of their high-compression ratio, had not been used extensively.

Mr. PIERSON, discussing the relative efficiencies of the large aeroplane and the large flying boat, said that the aeroplane suffered considerable disadvantages. For instance, the transport of wings and the provision of proper handling arrangements had to be thought of. At the same time, the landing speeds must be very much lower than those of the seaplane. These considerations tended to make the aeroplane heavier structurally; hence, as the size of machines increased, the flying boat would be better structurally than the large aeroplane. Mr. Pierson was not quite in agreement with some of Major Rennie's figures, in Fig. 1. It was shown that (*a*) at a mean weight of approximately 23,000lbs., travelling 500 sea miles, 440 gallons of fuel would be required; and (*b*) at a mean weight of approximately 40,000lbs., for 500 sea miles, 440 gallons of fuel would be required also. This gave a constant value of 1.14 sea miles per gallon of fuel, irrespective of the weight of the aircraft. From calculations made in connection with recent design work, however, he and his colleagues had found that an increase of about 45 per cent. in the total weight resulted in a decrease of approximately 40 per cent. in sea miles per gallon. Therefore, the range in the second case would be considerably reduced.

Wing-Commander MAYCOCK, dealing with Major Rennie's statement that "a boat is seaworthy if it remains so while on the water, and when in the air is airworthy, and is able to alight in a seaworthy condition and still be airworthy," said it might be more truthful to state, "a flying boat is unseaworthy if it becomes unairworthy when it gets into the air." He felt that the question of seaworthiness needed clarifying, and something definite should be laid down so that designers and all concerned might really know what the users of the boats were after. It should be borne in mind that there were two distinct conditions, the first applying to the machine when on the water, taxying or being towed, and the second applying to taking off and landing. Those two had no relation to each other at all. The latter was mainly a matter of aerodynamics; whilst the former involved questions of construction, buoyancy, handling, and freedom from damage by water. It was early yet to talk about taking off in rough seas. Possibly five or ten years hence we might be talking about it, but in order to accomplish it at present we were sacrificing more important qualities, *i.e.*, cruising speed and range. The majority of uses to which flying boats were put to-day necessitated the provision of bases, and the only possible occasions upon which flying boats would want to take off in rough seas would arise when they had made forced landings or when they were carrying out rather unusual military operations. All that was needed to-day was the ability to take off from and land on sheltered waters, whether in a light or over-loaded condition, and seaworthiness in rough weather was something to be aimed at later on. Another point, not raised in the paper, was that of providing facilities for getting alongside a seaworthy flying boat. At present we had flying boats which were so seaworthy that we could not get alongside them in rough weather, and it was rather hopeless to have such a seaworthy boat which we could not board. It was inherent

in the design of flying boats that the wings should be at right angles to the hull, but the designers should think about providing a little space to enable small boats to come alongside without damage. The question of seaworthiness in harbour had a great deal to do with what was called "seamanship," but which he preferred to call "marine airmanship," because he had suffered considerably from the "seamanship" of various sailors at times. With regard to the value of slots, he said that if we could get satisfactory slots on flying boats, no doubt they would be of very great assistance. Their principal value lay in the fact that they would give control, or at least stability, when the machine was thrown out of the water by a wave before full control had been obtained. With the "V" bottomed types of aircraft they would ensure a safe landing. Congratulating Major Rennie, he said the paper was one of the most lucid he had had the pleasure of reading; it not only set out the difficulties to be faced, but was also constructive, in that it pointed out the compromises we had to adopt in designing flying boats for really useful purposes, *i.e.*, long flights over the sea and carrying paying loads either of merchandise, passengers, or bombs.

Major BARLOW said there was no reason why, if the lines of development suggested by Major Rennie were followed, the flying boat should not fulfil the Empire requirements outlined by him at the end of the lecture. One outstanding feature was the reference to the relative importance of cruising speed and range. Normally the cruising speed of a flying boat was undesirably low. The maximum range in still air arose, in the main, from three conflicting factors: (1) the L/D of the whole machine; (2) propeller efficiency; and (3) petrol consumption. Considering these three factors independently, the L/D of the whole machine threw back cruising speeds, and Figs. 5, 6 and 7 showed this. The second factor alone would lead to high cruising speeds, so that we could get absolutely the best out of the propeller. The third factor alone would mean the present-day engine running at 9/10ths full throttle to obtain the best consumption efficiency. The results of all these factors in general brought us to a cruising speed which varied from 5 to 15 miles per hour above the best speed for L/D, which was usually so low as not to be a very practical flying proposition, taking into account the possible wind conditions on long ranges. In every case in the paper (except Fig. 14) the highest cruising speed was obtained at the expense of range. That meant that if one designed for a certain range, with a very high cruising speed, say 100 knots, there was always a machine with a lower cruising speed which would have a longer range. He did not think we need do much more in the direction of "fining" down the body, as the body resistance was relatively very small when flying at the best cruising speed. His own opinion was that the flying boat designers had attained to a very high efficiency in that respect, as shown by Major Rennie, and any improvements there would be in the near future were relatively small. But the carburetter designer was definitely at fault and behind in development by not giving a reasonable consumption figure over the whole throttle range. There was nothing more trying, after putting all one knew into the designing of aircraft and engines, than to have a carburetter which would throw 20 per cent. of it away in range. The best consumption must coincide with the best cruising speed to obtain the maximum all round efficiency so essential for a flying boat to do useful work.

Commander BRIGGS, as one who had been associated with aviation in the days when the very first flying boats were made—long before the advent of the Porte boats—and with the lecturer, Major Rennie himself, at a period some 12 years ago, but who had had little association with flying boats since those early days, said he was able to appreciate very fully the enormous progress that the lecturer's paper showed. He read the following communication from Mr. Rowledge, who was unable to be present at the meeting:

Mr. ROWLEDGE (*contributed*): I find Major Rennie's paper of particular interest and value, partly because the aeroplane largely quoted by him is fitted

with engines of which I have personal knowledge, and because the problems discussed in the paper cover ground of equal importance to both the machine and engine designer.

Major Rennie asks what are the possibilities in the direction of reducing fuel consumption. There are certainly possibilities in this direction, probably greater than the reduction quoted by Major Rennie as increasing the range by nearly 20 per cent. As far as the engine itself is concerned, it is already accomplished, but it remains a matter for discussion between the aeroplane builder, the pilot and the engine designer as to how the results can be obtained in practice.

Major Rennie also asks what is the service to be expected from an engine running for lengthy periods at from 90 per cent. normal revolutions to normal revolutions. In my opinion, this is the desirable speed at which an engine should be run if good service is to be obtained. A very slight drop in revolutions from normal with the engine throttled is much better than reducing the power output by reducing the speed and retaining high B.M.E.P. I believe this also enables better performance to be obtained from the aeroplane, particularly as regards take-off.

With regard to the variable pitch propeller, I think this should now be in sight, but if the problem remains unsolved, the two-speed gear is available.

Commenting on Wing-Commander Maycock's remarks with regard to the troubles which had arisen from the seamanship of naval officers, Commander Briggs expressed the hope that the combined efforts of the naval officers and " marine-airmen " would lead to the solution of this very difficult problem.

Major LEDEBOER (Chief Technical Officer of the Marine Experimental Station) congratulated Major Rennie upon the very great service he had rendered to seaplane development, not only by designing seaplanes, but by putting forward this paper, in which he had stated the first principles of the problem. So far as he knew, they had never been collated before, and it would be an immeasurable boon to all engaged in aeronautics if the first principles of every phase of the problem could be collated. He did not think anyone could quarrel with Major Rennie's technical statement of the problem, but he would pick a minor quarrel with Major Rennie with regard to the use of the term " cruising speed." In one part of the paper he had agreed that it was the same as " economic speed "; again, Major Rennie apparently considered it to be equivalent to " range speed," which was a totally different thing. The use of a loose term such as " cruising speed " in a technical paper was a very grave misdeed, because the term meant practically nothing except the speed selected by any particular pilot to suit his own convenience or comfort. Discussing petrol consumption, he said that for all he knew we might be in possession of accurate figures relating to petrol consumption in the air as distinct from the bench, but if that knowledge did exist it was by no means widely diffused, and he fancied that the figure of 0.5 or 0.45 which Major Rennie had quoted was based upon bench tests and not on practical conclusions. Certainly his experience did not bear out anything like that saving. If, by the use of appropriate air mixture control, the figure could be reduced to a material extent, as he believed was the case, it must not be left to the individual pilot to manipulate that control. The pilot would be far too busily engaged with other things; again, it would no doubt be agreed that the lightness of touch of the various pilots differed to some extent, and if the control were brought into being it should be automatic, in order to give the results expected of it. In a final reference to the value of the paper, he expressed the view that it was destined to become a classic in the literature of seaplane design.

Mr. OSMAN said the paper was almost an education to some of those present, for the whole problem had been presented in such a lucid manner. He hoped that the suggestions put forward would be followed up and would bear fruit in due course.

Squadron Leader HAIG, referring to Figs. 5, 6 and 7, in which the effect of aspect ratio on the range, etc., was indicated, asked whether the effect of variation in structure weight due to these changes in aspect ratio had been considered when making calculations.

Major Rennie had shown that as the aspect ratio was increased the range increased and the cruising speed dropped. If one continues the curve in Fig. 6 to show an infinite aspect ratio it would indicate that one would get an infinite range at a speed of somewhere about 40 m.p.h.

It was reasonable to assume from Major Rennie's figures that the rate of fuel consumed per mile flown varied with the aspect ratio; it should be possible, therefore, by combining the curves of the variation in wing weight due to change in aspect ratio and the variation in fuel consumed for any particular range at different aspect ratios, to get the best aspect ratio for that particular range; disregarding other considerations.

With regard to the shortcomings of the present day carburetters, he suggested that the use of compression ignition engines giving a consumption of about .36 lbs. per b.h.p./hour, as against the present day .5 lbs. b.h.p./hour; this will make it possible to increase the range of a flying boat considerably, particularly as the consumption per b.h.p./hour tends to come down as the engine is throttled when compared with the well-known increase in consumption of the petrol engine per b.h.p./hour when throttled under these conditions.

A further point was that Major Rennie had shown the importance of this improved fuel consumption, and this had a bearing on the aspect ratio.

With an engine consuming considerably less fuel than the present day petrol engine, one could afford to give away a certain amount of aerodynamic efficiency by further reducing the aspect ratio, thereby saving structure weight and getting back some of the increased weight of the engine.

Squadron Leader ENGLAND was of the opinion that slots utilised for lift, in taking off and landing, had as great, if not greater, value than they had for control. They had been tried out and had been worked automatically with success. He agreed with Major Rennie that the "F" boat was more seaworthy that the present day flying boat. There were two aspects of the problem of seaworthiness, (*a*) seaworthiness when moored up, and (*b*) seaworthiness from the point of view of landing and taking off. The question of approaching the boats when moored up, as mentioned by Wing-Commander Maycock, came within aspect (*a*). He agreed that it was difficult to approach a flying boat when moored in a rough sea, but that was a minor consideration as compared with being forced to come down on to the water out at sea and having to get off again. He had read, and had been told by pilots, of forced landings in enormous seas—in 8ft. seas, and so on—but he would like to know how they measured that 8ft. when landing, because he had seen the difficulties that beset the best of pilots when trying to get boats down on to a 6ft. sea. If the machine alighted on the water in one piece, the lower plane would be completely smashed up within twenty minutes. It seemed to him that we were tackling the problem of the seaworthiness of flying boats from the wrong aspect altogether. There were two things which went to make flying boats seaworthy, the first being the position of the lower plane relative to the sea, and the other the position of the engines and airscrews. The main difficulty with a flying boat was that of taking off with an adequate reserve of power; most could get off so long as the propellers were not broken before the machines were actually air-borne. But the alighting difficulty still remained, and if it were necessary to alight in a rough sea the lower plane soon became damaged. He had been horrified to hear Major Rennie say that a certain boat had a landing speed of 55 knots; that seemed to be courting disaster. It might be all very well when operating in sheltered

waters, where one did not experience rough seas. In his view the flying boat had possibilities greatly superior to those of the aeroplane providing it could operate from any stretch of water, but if the landing speed were very high, that fact would definitely preclude the flying boat from landing on rough seas. Referring to the cinematograph films exhibited by Major Rennie, he said he had noticed that the flying boats landed at a very flat angle indeed. From his experience gained with numerous flying boats during the war it had been proved conclusively that unless the actual boat itself could be brought right back to an angle of from 15 to 18 degrees it was not going to be seaworthy. It was entirely wrong, in his view, for flying boats to have a high stalling speed, because sooner or later they would be operating over ocean routes, where a long swell was experienced, and it was the long swell with which it was most difficult to contend when taking off and landing. If the boat had a sharp "V" bottom, as it left one wave and touched the next deceleration set in, and that affected the taking off, although it had every advantage from the landing point of view.

Mr. WYN EVANS: It is good to listen to another lecture on flying boats and one by so eminent an authority as Major Rennie. It is even better to think that the day of the really long range flying boat is in sight. The recent overseas flights to the Baltic and the present successful Far East cruise under Group Captain Cave-Browne-Cave surely indicate the approach of regular, rapid Imperial communications, efficiently performed, upon which, as Sir Sefton Brancker said some time ago, the future solidarity of the Empire may well depend.

As the previous speakers have dealt with the technical details of the paper, may I be allowed to say a few words on the general aspect. I believe that the deservedly greater development of the flying boat will ensue so soon as the real interest of the big shipbuilding companies and the marine community generally is aroused, quite apart from the aeronautical side of the question.

To further the attainment of—for want of a better term may I call the "universality" of the large flying boat, I have on several occasions advocated a certain concentration of design on an inherently stable hull unit. I say "unit" advisedly, because it is difficult to realise this in the present single hull. This inherently stable hull unit can, with wings removed, be treated and manœuvred as an ordinary surface craft. The idea is not fantastic. I was privileged last week to see a design of great interest, which I think will attain this object whilst at the same time retaining excellent airscrew clearance. At least two advantages will accrue—the first, the ability to dispense with the always inconvenient slipway; the second, the omission of wing tip floats, and all designers and pilots will welcome their exclusion. Incidentally, I understand that Sir Alan Cobham nearly lost his machine not so many hours ago through the holing and breaking away of one of these floats. With regard to the first of these advantages I visualise the removal, whilst afloat, of the wings on either side of the centre section to enable the boat to be docked, when necessary, in the normal way. This removal of wings can be carried out under shear legs or by dockside or floating crane, or merely by their being lowered and floated clear. This will render all the harbours and docks of the world immediately available for flying boats without the trouble and expense of additional equipment in the shape of slipways. Such a hull design will complete the universality and solve one at least of the problems of the long range flying boat.

We in the Airworthiness Department, at Farnborough, have been instructed recently to undertake an investigation into the strength of all-metal flying boat hulls, and we have already reached some interesting results. There is no possibility at the moment of detailing these results, but I hope sanction will be granted to present them in the near future. At the moment, the full co-operation of all designers of these hulls is being sought.

One or two points may be of interest. We feel convinced that a saving of weight can be effected in some of the existing metal hulls and that future designs will benefit accordingly. We also feel that greater attention should be given to unsupported plate area on the planing bottom, that this should be reduced to a minimum, especially where the form is practically flat. This could be achieved—in some existing designs with a reduction in structural weight—by introducing a larger number of small stringers from keel to chine in place of a small number of large stringers. Unsupported plate area seems to be a criterion of strength and seaworthiness of the planing bottom.

This possible saving of weight, coupled with the use of as long plates as possible, which will mean less laps, butts and rivets, could be employed in partly meeting the weight involved in the development of double bottoms. The incorporation of double bottoms, not at all popular in existing hulls, or alternatively, the splitting up of the forebody into a number of watertight compartments, seems absolutely essential in the development of the long range flying boat. One cannot always be certain of missing a piece of waterlogged timber, or partially submerged object when " taking off " or alighting, and this without some scheme of double bottoms might easily lead to the total loss of the craft.

I have referred to the subject of Major Rennie's valuable paper in a general way, rather to urge the development of the long range flying boat than to criticise the details of performance and endurance which have been dealt with by previous speakers and which must be considered as a basis of each individual design.

Captain NICOLSON said an interesting point was made in the paper, where the possibilities of increased range due to a lower consumption per b.h.p. was shown to be vastly greater than increased range due to reduction of parasite resistance. There was a great difference in the throttled consumptions of existing engines, ranging from a low figure of 0.46 pts./b.h.p./hour (R.R.F. XI.) to as much as 0.64 pts./b.h.p./hour (Jupiter VI. in Medina). These consumptions were obtained on bench tests, but owing to the engine nursing found necessary for low consumptions in the air, low figures were seldom met with under service conditions. It appeared to be up to the engine makers to make the next step in improvement. With regard to the fact that in Fig. 4 the thrust curve cut the total drag curve, and yet the machine was capable of getting off the water, he said this had been frequently noticed, and it emphasised that without full-scale experience of flying boat work a correct interpretation of model results or theoretical figures was impossible. He agreed that a multi-engined machine was superior to a twin or single. The " Valkyrie " was a case in point, it having flown on one occasion for an extended period with a wing engine failure. With regard to re-fuelling, he said that for some time he had been advocating re-fuelling from tank tenders by a system in which compressed air discharged the fuel from the tender direct to the machine under a pressure sufficient to deliver 1,000 gallons in half an hour or less. He had great belief in the future of large flying boats, of much larger size than at present contemplated, especially for civil work. In spite of the many statements he had heard to the contrary, he was of opinion that the larger the boat the greater its seaworthiness.

Mr. LANKESTER PARKER asked Major Rennie if weight had been taken into full account when considering the effect on general performance of varying the landing speed from 45 to 55 knots. It would appear that there would be a very considerable increase in structure weight in the former over the latter.

He agreed with Wing Commander Maycock when he said that there should be more than one type of seaplane, for instance a machine for use on the open sea should be expressly designed for taking off from and alighting on rough waters, whereas a more efficient aircraft, with perhaps a higher landing speed, could be produced for work over comparatively calm waters. This, it seemed

to him, is the only type of seaplane that can fairly be compared with its contemporary land machine, for after all the land machine often flies over country upon which it cannot alight without damage.

Captain W. H. SAYERS was very interested to find Major Rennie, himself a designer of modern flying boats, stating that the hull form of the old Felixstowe "F" boats was superior to those of many of their modern successors. He had heard many experienced pilots make the same statement in private, but it seemed to be considered indiscreet to announce the opinion publicly. According to Major Rennie, the time to take off varied as the square, and the distance as the cube, of taking off speed. In an article by Mr. Gouge, published recently, it was said that the time was nearly independent of the take off speed, and the distance directly proportional to that speed. This apparent inconsistency, Captain Sayers assumed, was due to the fact that Major Rennie was considering a normal take off, whereas Mr. Gouge was dealing with a limiting case of a boat loaded so that it could only just take off. For the latter case the curves (of time and run, against speed) straightened out. Discussing Major Ledeboer's suggestion that, in order to ensure uniform results, the carburetter mixture control must be automatic and not subject to the pilot's discretion, Captain Sayers said he understood that Imperial Airways had lately fitted a cam on the throttle of certain engines which operated the altitude control and secured the most economical mixture over a wide range of throttle setting. This had had the effect of very considerably increasing the practical range of the machines so fitted.

Mr. FEDDEN (*contributed*): I have read the proof of Major Rennie's paper with much interest; firstly, because I consider the air-cooled radial type of engine to be essential to the real solution of his problem and, secondly, because of the informative nature of the paper; the fact that it is presented by an authority with considerable practical experience of his subject, and his evident desire for a closer liaison between the aircraft and aero engine designer, a point which I have myself frequently stressed.

In view of the above points and Major Rennie's direct invitation for statements from the engine side, I much regret my inability to take a personal part in the discussion.

Proceeding progressively through the paper and leaving strictly aerodynamic considerations in other and more competent hands, I have the following comments to make.

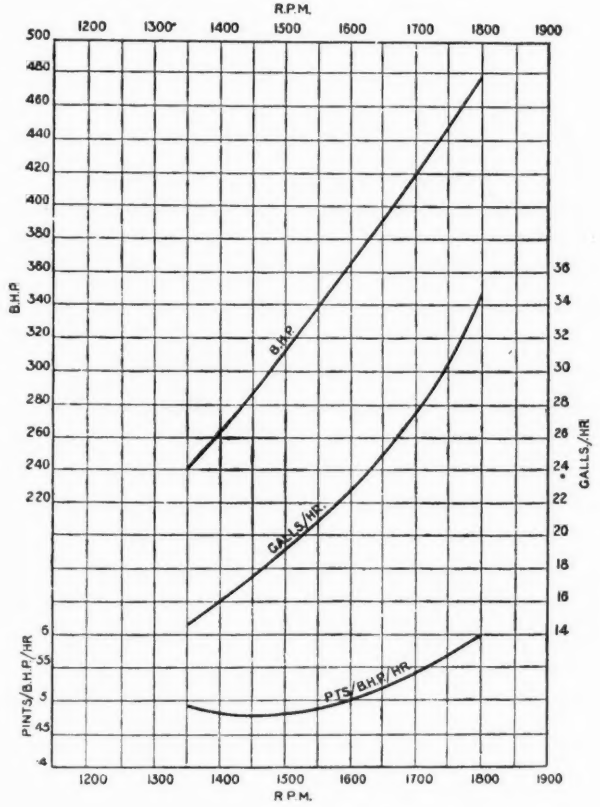
(1) In diagrams 5, 6 and 7, referring to the case of a three-engined flying boat, with a 450 b.h.p. air-cooled engine, I presume that the author had in mind a Jupiter Series VI. low compression engine. If so, and in view of the curves following dealing with a high compression water-cooled engine, and the natural tendency to draw comparisons, I should like to emphasise this, and to point out that the Jupiter consumptions are evidently based on a conventional throttled curve and were taken without the use of the mixture corrector, which in a case of this kind is such an important feature of the Bristol Triplex carburetter fitted to this engine. Suitable operation of this corrector results in a fuel economy of 25 to 40 per cent., at the comparatively easy cruising conditions existing on a long range flying boat, particularly in the later stages.

Further, with a 6.3 compression engine the results would be better still. Considerable experimental and development work has been done on air-cooled engine consumptions during the last few months. The accompanying chart will show what can be given with the 6.3 engine.

I admit that it is only comparatively recently that we have concentrated on the problem, and that the information we have obtained from our bench and flight tests is still in process of circulation.

(2) With regard to the engine consumption when throttled and its effect on range, I am only too pleased to be able to assure the author that consumptions of 0.5 to 0.45 pts./b.h.p./hour can be given at present with the Jupiter engine for the cruising conditions obtaining on a long range flying boat.

I should like a little more information from the author on his remarks in connection with engine revolutions.




I do not know what he would call a lengthy period, but I would point out that British Service aero engines during their type test are required to run 100 hours, in 10-hour non-stop stages, at normal r.p.m., and in the case of the Jupiter, many long non-stop runs up to 50 hours' duration have been carried out at normal r.p.m., and recently the Italian Government have carried out a 100-hour non-stop type test on the Jupiter at 420 h.p. and normal r.p.m.

I presume that his statement that "an engine which has to be nursed at low revolutions is useless for long range flights" should be taken as "an engine which has to be nursed by being run at low revolutions."

In conclusion, I feel that the air-cooled radial has a particular application for flying boats and seaplanes, owing to its light weight and simplicity of installation, its ability to taxi long distances without overheating, and the facility with

which it may be got away from its moorings, owing to its ease of starting and quickness in warming up to normal operating temperature.

The latest type Jupiter, with a 2:1 reduction gearing, is a further stage forward in our endeavour to comply with the many requirements of the aircraft designer, and has resulted in a greatly improved performance with machines of the type dealt with by the lecturer.

I should be pleased to have the opinion of the lecturer on the tandem pusher type of installation which, in the case of the Jupiter, is meeting with great success on the Continent, some half-dozen new installations on large seaplanes or flying boats being at present under test.

The C.A.M.S., with two Jupiters in tandem, has recently put up a record for seaplanes of distance and height.

The Dornier Super Wal, undergoing tests at Lake Constance at present, with four Jupiters in tandem, has put up a remarkable increase in performance over the water-cooled engined machines.

Finally, I would like to thank Major Rennie for the information given from the aircraft designer's view point, which I, for one, find of great help to the aero engine designer in his endeavour to supply the right goods to the right market.

Speaking for the Bristol Company, I should like to take this opportunity of assuring all aircraft constructors that we are only too pleased to give them full information, according to their particular case and problem.

Mr. SPANNER (*contributed*): It is a pleasure to read Major Rennie's sound technical opinion on this particular problem. But what a thousand pities that it should be the case that such a paper as this should be foredoomed to receive far less notice than is given the vapourings of non-technical enthusiasts, some of whom occupy official positions for which they are unfitted.

If aeronautical engineers will set themselves seriously to undertake the task of stifling the voices of their ignorant friends, civil aviation will receive support at present denied to it. Marine interests are not asleep nor, I believe, are they altogether reluctant to afford the help and sympathy for which Major Rennie pleads so ably, but before that assistance is forthcoming it is essential that the "Riot Act" should be read to certain Press "Air Correspondents," and also to all those "air-minded" cranks who persist in flooding the papers with air propaganda having no technical bottom to it.

The Royal Aeronautical Society would do aviation, both civil and military, a great service if it set up, within itself, a Technical Committee whose duty was to combat the flood of ill-informed air propaganda which floods the daily Press, some of it from official and semi-official quarters.

Major Rennie is commendably frank when he admits that "of all means of transport, the consequences arising from even a minor breakdown in power plant or structure are likely to lead to a more serious loss in life and material in the case of air than in the more accepted forms, such as ship, rail or road transport." Progress is made by facing such facts honestly—not by ignoring them and devoting one's energies to the visualising of a future in which aircraft override a hundred and one technical obstacles which at present loom very large to the engineer of sober judgment.

I have a considerable belief in the immense importance to this Empire of aircraft of moderate range, airplanes and seaplanes—particularly the former—but I find myself, at every turn, confronted with the fact that experienced engineers, not connected with the aircraft industry, are inclined to look upon all interested in that industry as "hot air merchants." They judge of the solid worth of the aircraft industry by what is said by non-technical heads of certain

Government departments—and aeronautical engineers know of what little value such pronouncements really are.

A reasoned paper such as Major Rennie's, on the other hand, tends to establish confidence.

Will Major Rennie please define for us the term "rough sea" which he uses. It may be difficult rigidly to determine a standard, but some definition is necessary as it is obvious that there is a decided limit to the possibility of coming down with safety.

Could we also have information as to the limiting conditions of wind and sea under which it would be possible for a seaplane to ride safely at anchor off a lee shore? I am impressed by the fact that a seaplane cannot carry much in the way of anchor and mooring cables or wire. At the same time she has a large wind surface—factors which together would make her plight extremely serious in certain circumstances. Perhaps Major Rennie is able to furnish reassuring evidence on this point.

Is the slotted wing device likely to prove as valuable in seaplanes as in airplanes? I raise the point because I believe there are certain limits to the landing angles in the two cases. Further, assuming both machines had slotted wings, would a forced landing on a moderately unsuitable surface by an airplane be more, or less, risky than a forced landing by a seaplane in a moderately rough sea? I hope Major Rennie will get the meaning of my question.

One final question. Is there any difficulty in landing on the water, assuming it moderately calm, in fog or in a bad light?

Flight-Lieut. COMPER (*communicated*): In associating myself with so many who had the pleasure of listening to Major Rennie's extremely interesting and instructive paper, I would like to put forward what seem to be to be alternative aspects of the problem of the long-range flying boat.

The lecturer has shown that the big gain in range will come with aerodynamic advance and carburetter improvement. Reduction of load factors, as another means towards the same end, has also been suggested but, very naturally, with little enthusiasm. In fact, as seaworthiness will be, in the main, a product of structural robustness, progressive steps will lie in a general increase of load factors.

Now, if range is essentially non-stop flight, the corollary should be that non-stop flight is a function of range. To our cost, we know that it is not so. Considering the numerous causes that terminate the least ambitious of non-stop flights before the aircraft's tanks are empty, it surely can be concluded that the prime factor in long distance flying is the acceptance of the necessity for (and provision against) temporary descent for asylum from adverse weather, or for repair of defect.

For the sake of argument then, we can assume that the Atlantic crossing can be brought within the range of the present-day marine aircraft as outlined in Major Rennie's paper, but the fact has still to be faced that a forced descent is made none the less likely, or less hazardous, by such artificial boosting of range.

Would it not be in the better interests of aviation to aim to increase the range of flying boat services by a careful choice of routes which can provide refuelling bases and protected waters? Then development can proceed on substantial lines in that prime consideration will be given to questions of seaworthiness, load carrying capacity and other all-important qualities.

As has been shown by Major Rennie, many of these qualities will have to be sacrificed if range, in terms of non-stop flight, is alone to be considered.

The CHAIRMAN asked Major Rennie if he would take the figures he had given on engine failures in the last part of his paper and re-cast them on the basis

of utilising the same engine fitted with a variable pitch airscrew. After all, the situation was substantially altered as between the twin and the three-engined machine if variable pitch airscrews were used, and as the variable pitch airscrews were coming along, and one was likely to be in the air soon, it was desirable to have that information. The question of the use of wet and dry docks and general facilities did not arise, strictly speaking, out of the title of the paper. The question of seamanship was one of very great importance indeed, and Major Rennie, having been intimately concerned with the operation of flying boats for many years, knew absolutely what he was talking about in stressing this point. Finally, reverting to the comparison of the flying boat and the aeroplane, the Chairman said that if it were accepted as a fact, and he thought it must be, that the flying boat had received very little attention in comparison with the enormous amount of attention paid to the aeroplane, the present state of development of the former was not so very unsatisfactory and great things might be expected of it.

Proposing a vote of thanks to Major Rennie for his paper, the Chairman said it brought our knowledge of the subject up-to-date, and would be a valuable contribution to the Proceedings of the Society.

The vote of thanks was accorded with acclamation.

Major Rennie intimated that, as most of the questions were of a technical nature, he preferred to reply in writing.

## Reply to Discussion

With regard to the question of engine failure as between the twin and the three-engined aircraft, if the former was fitted with variable pitch propellers. I regret I have been unable to look into this, so I cannot give a definite answer.

In reply to Major Buchanan, while I quite appreciate he has access to all official records of the performance of aeroplanes and flying boats, I venture to say he has not made use of the latest information available. So far as I am aware, there is no aeroplane corresponding in size and to a similar specification to the Blackburn "Iris." However, if he compares the "Iris" II. with, say, the "Argosy" or the "Hercules" I am sure he will find this flying boat to have an aerodynamic efficiency equal, if not superior to either of these aeroplanes.

With regard to the question of seaworthiness when comparing many modern flying boat hulls with the "F" boats. The term is restricted in this instance to seaworthiness as effected by the form of the forebody, and I think if Major Buchanan will compare the forebody of the "Iris" with the "F" boats, including the "Fury," the line of development will be seen clearly.

I am afraid I do not follow Mr. Pierson's remarks on Fig. (1). Fig. (1) applies to a flying boat normally designed for a gross weight of 29,000lbs. and the curves show the range as fuel is added as an overload. Calculations and actual consumption tests show that for a given aircraft the air miles per gallon at cruising speed varies nearly linearly with the weight of the aircraft, so it is quite correct, when estimating the range, to use the value of the air miles per gallon corresponding to the weight of the aircraft with half fuel.

I have to thank Wing Commander Maycock for his interesting and suggestive remarks, and for his appreciation of the paper.

Mr. Rowledge has kindly given the answers to my queries on the possibility of a reduction in fuel consumption and on the most desirable speed at which an engine should run. The latter information should be of great use to all those concerned in aircraft operation. It may interest Mr. Rowledge to know that since this paper was written consumption tests have been carried out on "Iris" with and without mixture control. The tests show that at normal load the cruising

speed has been increased by eight m.p.h. and the air miles per gallon from a value of 0.9 to 1.05 by the use of mixture control.

In reply to Major Ledeboer, I have to thank him for his appreciation of the paper. With regard to the definition of cruising speed, this I defined at the outset to mean the most economical speed, that is, the speed at which the air miles per gallon are a maximum.

I think Squadron Leader England will find I have nowhere in the paper advocated a landing speed of 55 knots. On the other hand, there are conditions under which a boat may have to operate, which would permit of even a higher value than this.

With regard to the trim back when landing, I think it is somewhat difficult to judge the angle from a photograph. The boat shown on the film may be trimmed back to over 14° wing incidence.

Commander Wyn Evans raises several points not included within the scope of the paper, to which I regret I am unable to reply on this occasion. With regard to the strength determination of hulls, we investigated this question several years ago, and have developed methods of sufficient accuracy to enable a consistent design to be obtained.

In reply to Mr. Lankester Parker, the calculations on the performance, varying the aspect ratio only, were carried out for a boat of 17,000lbs. gross weight, as stated. As it was desired to ascertain the effect of the aerodynamics on the performance, any difference in structure weight due to aspect ratio would merely alter the useful load carried. I do not altogether agree with Mr. Parker that flying boats should be designed to operate either in rough or calm water, but not both, as, apart from the question of landing speed, a hull designed for rough water is not necessarily less efficient than one designed solely for calm water conditions.

I have to thank Mr. Fedden for his interesting remarks on fuel consumption, and the engine revolutions which may be maintained over lengthy periods. My statement as to the nursing of engines is probably not quite clear, Mr. Fedden's interpretation is what I mean.

In reply to Mr. Spanner, I realise it is most difficult to define a rough sea, because a rough sea is here meant rough so far as a flying boat is concerned. In my own mind I picture a sea with waves about four to five feet high.

With regard to the limiting conditions of wind and sea under which it would be possible for a flying boat to ride safely at a mooring, I would say that the "Iris" II. rode out a gale at Felixstowe, the wind velocity reaching a maximum of seventy miles per hour.

Providing night landing apparatus is fitted, successful landings may be made in a thin fog or at night.

I agree with Flight-Lieut. Comper that for long range civil flying boats the most economical solution is to provide suitable refuelling bases in order to increase the paying load. On the other hand, for military purposes non-stop long range may be a necessity.

# PROCEEDINGS

## First Meeting, Second Half, 63rd Session

A meeting of the Royal Aeronautical Society was held in the rooms of the Royal Society of Arts, John Street, Adelphi, London, W.C.2, on Thursday, January 5th, 1928. Colonel the Master of Sempill (President of the Society) was in the Chair, and Mr. A. Fage, of the National Physical Laboratory, read a paper on "Some Recent Experiments on Fluid Motion."

The Chairman, opening the proceedings, said that Mr. Fage needed no introduction, because it was well known that he was one of the first people engaged on aeronautical work at the National Physical Laboratory, and he was one of the Senior Assistants there. He had started aeronautical work about 1912, and was the author of many of the reports and memoranda of the Aeronautical Research Committee.

## SOME RECENT EXPERIMENTS ON FLUID MOTION

BY A. FAGE, A.R.C.SC., F.R.AE.S.

### *Foreword*

The purpose of the present paper is to present to the Society the results of some experimental researches on fluid motion recently conducted in the Aerodynamics Department of the National Physical Laboratory. Two separate series of experiments are considered; so that for convenience of presentation the paper is divided into two parts. Part I. deals with the flow at the nose of low-resistance forms (aerofoil and elliptic cylinder) moving through both inviscid and viscous (air) fluids; whilst Part II. is concerned with the flows behind high-resistance obstacles immersed in an airstream. Both series of experiments were made for two-dimensional motion, a condition which obtains at the median section of a body of uniform (but relatively small) cross section when it extends between the walls of a wind tunnel. In view of the fact that the researches are fully described elsewhere* attention has been mainly directed in the present paper to a consideration of the results obtained, rather than of the experimental methods adopted. Also, in accordance with custom, the results are presented in non-dimensional form. The numbers given in the footnotes refer to the List of References given at the end of the paper.

It is a pleasure to mention that the researches in Part II. were undertaken in collaboration with my colleague, Mr. F. C. Johansen, B.Sc., and that assistance in carrying out this experimental work was ably given by Mr. J. H. Warsap. Valuable criticisms and suggestions were also kindly made by Prof. Bairstow (Part I.) and Dr. H. Lamb (Parts I. and II.) during the preparation of the original papers describing the researches.

### Part I.

### *On the Viscous and Inviscid Flows Around Aerofoil Forms*

(1) *Introduction.*—The general nature of the flow pattern at some distance from an aerofoil of infinite span advancing at a low incidence through still air is now well understood. An essential feature of the flow is that the average velocity above the aerofoil is higher than that below it, or in other words, there is a cyclic motion or circulation around the aerofoil. This cyclic motion is essential for the production of lift, and forms the basis of modern aerofoil theory.† For

* (7) Part I., (16) and (17) Part II.

† (1)—(4).

two-dimensional motion through a perfect fluid there is, as shown by Kutta and Joukowski, a definite relation between the circulation $\kappa$ around a contour enclosing the aerofoil and the lift experienced by the aerofoil. This relation is lift, $L=\rho V_0 \kappa l$, where $\rho$ represents the fluid density, $V_0$ the velocity relative to the undisturbed fluid, and $l$ is the length of the aerofoil. The circulation $\kappa$ is taken in the usual hydrodynamic sense as the line-integral of the velocity, that is $\int V_c ds$ around the contour, where $ds$ is an element of length of the contour, and $V_c$ is the component of velocity along this element. Messrs. L. W. Bryant* and D. H. Williams have demonstrated experimentally that this relation also holds with good accuracy when an aerofoil is advancing, at an angle of incidence below the critical value, through a viscous fluid such as air, provided that the contour line around which the circulation is measured does not approach at any part too closely to the aerofoil, and also that it cuts the trailing wake approximately at right angles to the direction of motion of the aerofoil. There are, however, important differences between the viscous and inviscid flows near the surface of the aerofoil even although the circulations taken around contours at some distance from the aerofoil are the same. In the absence of viscosity, the fluid slips at the surface and the circulation around any contour enclosing the aerofoil, including that coincident with the periphery of the aerofoil, is constant, whereas when viscosity is present the slipping at the surface is prevented† so that the circulation taken around the boundary of the aerofoil is zero. Further, with viscosity there is a trailing eddying wake and the aerofoil experiences a drag as well as a lift.

Although there are, then, drastic differences at the tail of the aerofoil, it might at first be supposed that when the lift was the same the two flows (viscous and inviscid) would have a common stagnation point at the nose. For at the stagnation point the fluid is stationary, and the viscous forces, which are proportional to the velocity gradient, are small. It will now be shown that experimental evidence‡ does not support this supposition.

(2) *Determination of stagnation points.*—At the outset, the position of the forward stagnation point measured on an aerofoil immersed in an air stream is compared with that for an inviscid flow having an equal circulation. Throughout the analysis it will be assumed that when the aerofoil is in an air stream the circulation is taken around a large contour which cuts the wake at right angles to the general direction of motion, so that the Kutta-Joukowski expression lift $=\rho\kappa l V_0$ holds; and further, the circulation $\kappa = K_1 \rho l C V_0^2 / \rho l V_0 = K_1 C V_0 = K_1$, if the chord $C$ and the undisturbed velocity $V_0$ are each taken as unity. When, therefore, we say the circulations measured in the viscous and inviscid flows are the same, it follows that the lift coefficients are also the same.

(3) The forward stagnation point for a body immersed in a fluid stream is the point where an approaching streamline meets the surface at right angles. At this point the velocity in the fluid is therefore zero, and since compressibility can be neglected, the pressure there is a maximum and equal to $(p_0+\frac{1}{2}\rho V_0^2)$. The position of the stagnation point for two-dimensional airflow§ can therefore be readily determined from observations of pressure taken at a large number of closely-spaced holes in the nose of the median section of an aerofoil when it is mounted between the walls of a wind-tunnel. The chord of the aerofoil selected was 6in. and the average distance between the holes was 0.008 chord, *i.e.*, 0.05in. approx.

(4) The position of the stagnation point for an inviscid flow could not unfortunately be obtained directly, since the solution of the equation $\nabla^2\psi=0$ for the aerofoil section chosen was not known. This position was therefore derived from diagrams of the flow pattern plotted in an "Electrical Tank," designed by Mr. E. F. Relf.‖ The principle of the tank depends on the fact that the streamlines for an inviscid flow and the equipotential lines in an electrical field are

* (5). † (6). ‡ (7). § See (5) and (9) for experimental proof that the flow is two-dimensional.
‖ (8).

identical when the boundary conditions correspond. Two sets of diagrams were taken, first that of the streamlines without circulation, and second that of the streamlines for circulation alone. A diagram of the flow pattern with circulation —from which the position of the stagnation point was determined—was obtained from the superposition of these two types of diagram.

(5) *Wind-tunnel results.*—The results obtained from the wind-tunnel are given in Fig. 1 (full line), where values of the ratio of the peripheral distance ($S$) of the stagnation point from the trailing edge to the chord length ($C$) are plotted against the angle of incidence ($\alpha$). As the angle of incidence increases, the stagnation point moves around the nose towards the under surface at a fairly uniform rate, until the critical angle is reached; then the stagnation point has an abrupt shift in the opposite direction, but afterwards continues to travel in the same direction as formerly, although at a slower rate. It will be noticed that

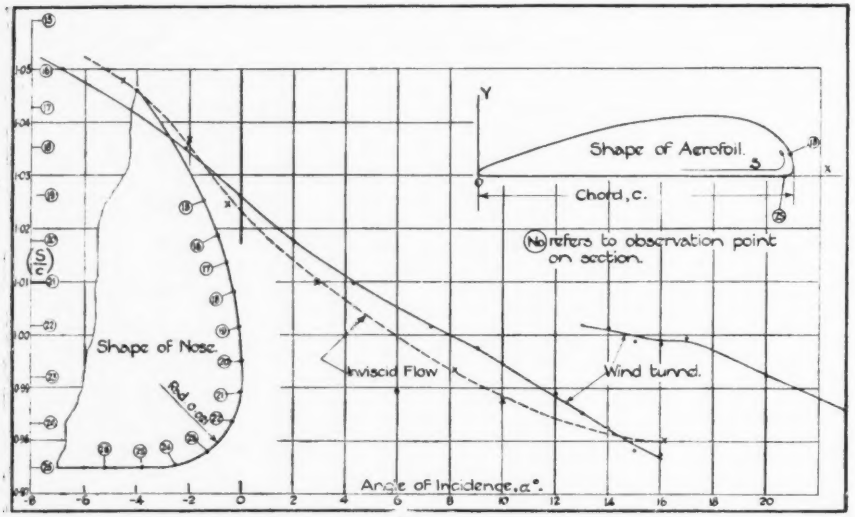



FIG. 1.

when the flow breaks down ($\alpha = 16°$) the stagnation point travels from the point No. 25 to the point No. 22 (approx.), so that since this part of the section is practically a quadrant of a circle, the zero streamline—which is at right angles to the aerofoil surface at the stagnation point—turns almost through 90°. It is also of interest to observe that two distinct positions of the stagnation point have been measured at each angle of incidence in the critical region ($\alpha = 13°$ to $\alpha = 16°$).

(6) *Comparison of theoretical and experimental results.*—The dotted line of Fig. 1 gives the movement, with change of incidence, of the stagnation point on the aerofoil in an inviscid fluid, when the flow at each angle of incidence is such that the circulation around a large contour is the same as that which would be measured around the same contour in the wind tunnel. A comparison of this curve with that drawn through the wind-tunnel observations indicates that, except at $\alpha = -1°$, the position of the stagnation point for the inviscid flow does not agree with that determined in the wind tunnel. Conversely, when the positions of the stagnation points for the two flows are coincident, at the same angle of incidence, the values of the circulation (and lift coefficient) are different. These differences are shown in Fig. 2, where values of $k_L$ for both the inviscid and

wind-tunnel flows are plotted against angle of incidence. Except in the region of $\alpha = -1°$ (where the drag is small) the differences are large, especially at extreme angles of incidence, both positive and negative.

(7) We shall now show that the differences exhibited between the two curves of Fig. 2 are not inconsistent with those which would be expected from the fact that in the case of the inviscid flow there is no eddying wake at the tail, and consequently no drag. Earlier wind-tunnel experiments* on the measurement of the total-head losses in the wake of this aerofoil have shown that at an incidence of $-6°$ the air soon after passing the nose breaks away from the under surface, and that a large part of the drag arises from a region of low pressure caused thereby. This pressure acts on the flat under surface, and so for the purpose of a general discussion may be regarded, in so far as the forces on the aerofoil are concerned, as equivalent to a downward normal force of magnitude approximately equal to $K^1_D \rho . l V_0^2 \operatorname{cosec} 6°$, where $K^1_D$ has probably the same order of magnitude as that of the measured drag coefficient ($K_D$). The lift com-

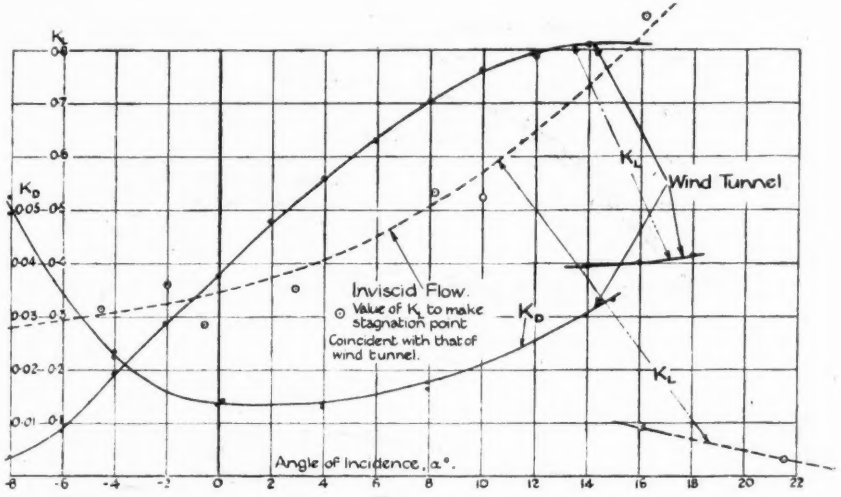



FIG. 2.

ponent of this force is $-K^1_D \rho . l V_0^2 \cot 6°$, so that the corresponding lift coefficient is $-0.329$, if the measured value of $K_D$ (0.0345) be substituted for $K^1_D$. Now supposing it were possible to superimpose (without altering the flow at the stagnation point) this lift component of the "wake" on the lift coefficient (0.295) calculated for the inviscid flow having the same stagnation point† as that measured in the wind tunnel, the resultant lift coefficient would be considerably reduced (from 0.295 to $-0.034$) and at the same time brought into closer agreement with the measured value (0.085). A still closer agreement would probably be obtained if allowance could be made for the part of the wake which is associated with the upper surface.

In the region of minimum drag ($\alpha = 0°$) the wake is narrow and related, more or less equally, to both the upper and under surfaces, so that its effect on lift should be small. That this is the case is shown in Fig. 2, where the lift coefficients for the viscous and inviscid flows are in close agreement. As the incidence increases, the wake arises largely from the flow over the upper surface, and so should make a positive contribution to the lift—that is, the wind-tunnel

* (9). † Situate near point No. 16 on the upper surface.

lift coefficient should be greater than the theoretical value. Fig. 2 shows this to be the case except in the region of the critical angle, where apparently the argument does not hold. Above the critical angle, the wind-tunnel flow breaks down and obviously should not be compared with a " streamline " flow. Calculations for the inviscid flow at a large angle of incidence have, however, been made and these show (Fig. 2) that the lift coefficient, when the stagnation point occupies the same position as that determined in the wind tunnel, is very small. In other words, the motion of the air in the region of the stagnation point (now

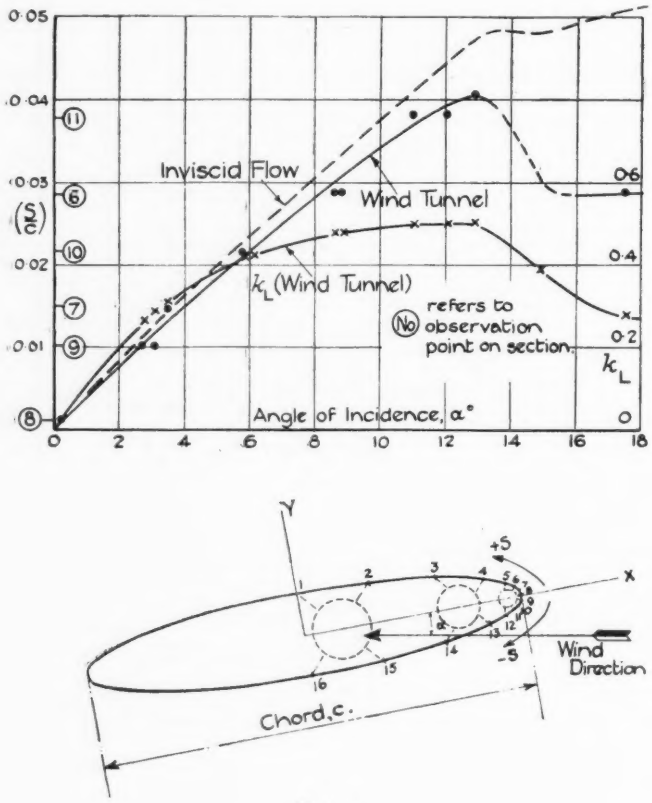



FIG. 3.

situated on the under surface) closely resembles that for an inviscid flow without circulation—a result not unexpected, since in practice a large part of the lift is probably associated with the region of negative pressure which is known to exist above the upper surface of an aerofoil inclined at a large angle to the wind.

(8) *Elliptic Cylinder.*—Further evidence in support of the general conclusions arrived at from the work on the aerofoil was obtained from a similar series of experiments made on an elliptic cylinder of large eccentricity. This model was selected because the position of the stagnation point for an inviscid flow could be determined mathematically,* and also because the form approximates to that of

* (10).

an aerofoil. Some of the results obtained with this model are given in Figs. 3 and 4. The full line of Fig. 3 exhibits the wind-tunnel movement of the stagnation point with change in the angle of incidence; whilst the dotted line gives the theoretical movement, when the circulation taken at each incidence is that which makes the theoretical lift coefficient equal to that measured in the

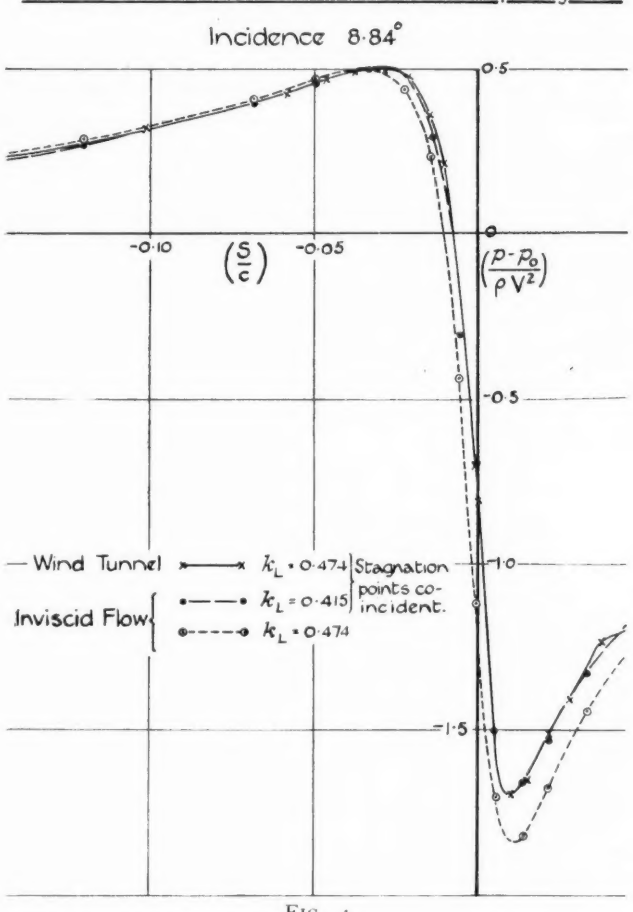



FIG. 4.

wind tunnel. It will be observed that the wind-tunnel and theoretical stagnation points do not occupy the same position; and further that the distance between these two points increases progressively as both the angle of incidence and the lift coefficient increase. The differences are such that the lift coefficient for an inviscid flow is smaller than that for the wind-tunnel flow at the same incidence, when the two stagnation points are coincident.

(9) Fig. 4 gives a comparison of the pressure distribution around the nose, measured in the wind tunnel with the elliptic cylinder at an angle of incidence of

8.84°, with those predicted for two theoretical flows which are related to the wind-tunnel flow in such a manner that in one case the stagnation points coincide in position, and in the other the circulations are the same. It is of interest to observe that the wind-tunnel pressure distribution not only in the immediate neighbourhood of the stagnation point, but also over a large part of the nose, is more closely represented by theory when the value of the circulation taken in the calculations is that which makes the stagnation point coincide in position with that measured in the wind tunnel.

## PART II.

### *On the Flow Behind High-Resistance Obstacles*

(10) *Introduction.*—The general character of the two-dimensional flow behind a high-resistance obstacle (that is, an obstacle which has the "dead-air" region at the back associated with discontinuous flow) immersed in an air stream has received considerable attention from a large number of workers. It is known that at the after end, or edges in the case of a flat plate, two vortex sheets are generated, and these as they flow downstream separate the freely-moving stream from the dead-air region at the back of the obstacle. At some distance behind, these sheets break up into two trails of discrete vortices of opposite direction of rotation to form what is now called a vortex street.* The vortices in the street are arranged alternately so that each vortex in one trail is midway between two successive vortices in the other trail. Photographs of this type of flow have been taken by various people; and we would here refer the reader to the beautiful pictures shown to the Society last May by Dr. Prandtl.† The general arrangement of vortices is illustrated diagrammatically, for the case of an inclined flat plate, in Fig. 5.

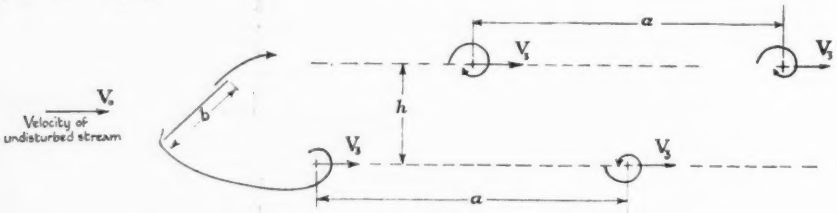



FIG. 5.

(11) The flows considered in the present paper are those created by a sharp-edged flat plate inclined to the general stream, and by the more complex forms of an aerofoil at a large incidence, a cylinder, and wedges of uniform but different cross-section. In each case, the obstacle was mounted between the walls of a 7ft. wind tunnel, and the observations were taken in the plane midway between the walls, where the motion proceeds in two dimensions.‡ For the purpose of experimentation it was convenient to regard the motion behind each obstacle as passing through three distinct stages; first, the shedding at the after end of the obstacle of the vortex sheets; second, the breaking-up of the sheets into individual vortices; and third, the vortex street régime at some distance behind the obstacle. It was found that the motion in the first stage was comparatively steady near the obstacle and that reliable observations could be taken which would allow an examination to be made of the structure of the vortex sheets. The periodic motion in the third stage was also of a uniform character and so could be subjected to an experimental examination. The transitory motion in the second stage was, however, too disturbed to allow reliable observations to be taken and so has not been considered.

* See (11), (12), (13). † (14), see also (15) and (17). ‡ See (5) and (9).

The more important results obtained from the researches will now be given. Although some overlapping with the work of other investigators is inevitable, and in fact desirable, it is hoped that novel aspects of the subject, in some respects at least, are presented in the paper.

## *Vortex Sheets*

(12) We shall commence with a study of the results of experiments undertaken to obtain some general ideas on the structure of a vortex sheet. For this purpose, attention will be given at first to the sheets shed from the sharp edges of a flat plate (breadth=6in.) mounted normal to an air stream, that is, a simple form of bluff obstacle; and afterwards to the sheets shed from more complex obstacles.

(13) *Boundaries of sheet* (*flat plate* $\alpha=90°$).—Since the vortex sheets shed from the edges of the plate separate the freely-moving stream from the more or less motionless air at the back, there must, if the breadth be small, be a steep gradient of velocity across the sheet. It is therefore to be expected that an indication of the positions of the boundaries of the sheet should be obtainable from velocity observations taken in the wake behind the plate. To determine whether this is so, explorations of velocity were made along several transverse lines taken close behind the plate and in a region where, as will be shown later,* the motion was comparatively steady. These observations were taken with a small hot-wire velocity meter shown on the left in Fig. 6. In use,

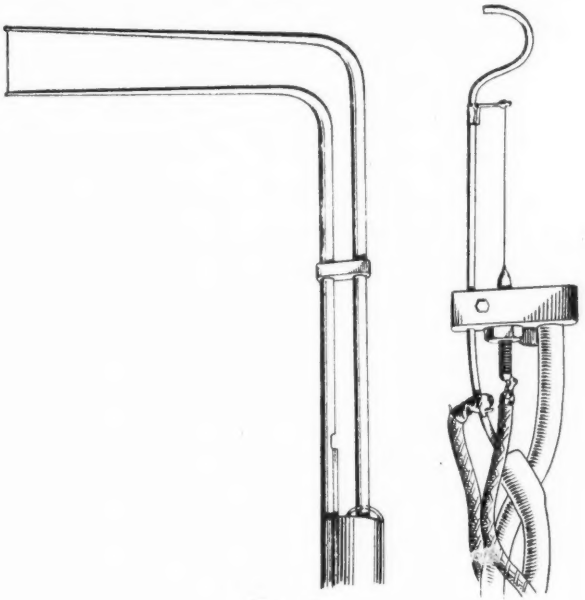


FIG. 6.

the hot-wire was mounted parallel to the edge of the plate, that is, normal to the plane of the two-dimensional flow. A complete description of this instrument is given in the paper referred to in the footnote.†

* (§ 17). † (18), also (17).

(14) The curves obtained from the explorations when faired values of $(V/V_o)$ are plotted against $(y/b)$—where $V$ is the velocity at a point in the field, $y$ the lateral co-ordinate of the point and $b$ the breadth of the plate—are given in Fig. 7. The outstanding feature of these curves is that they show very clearly

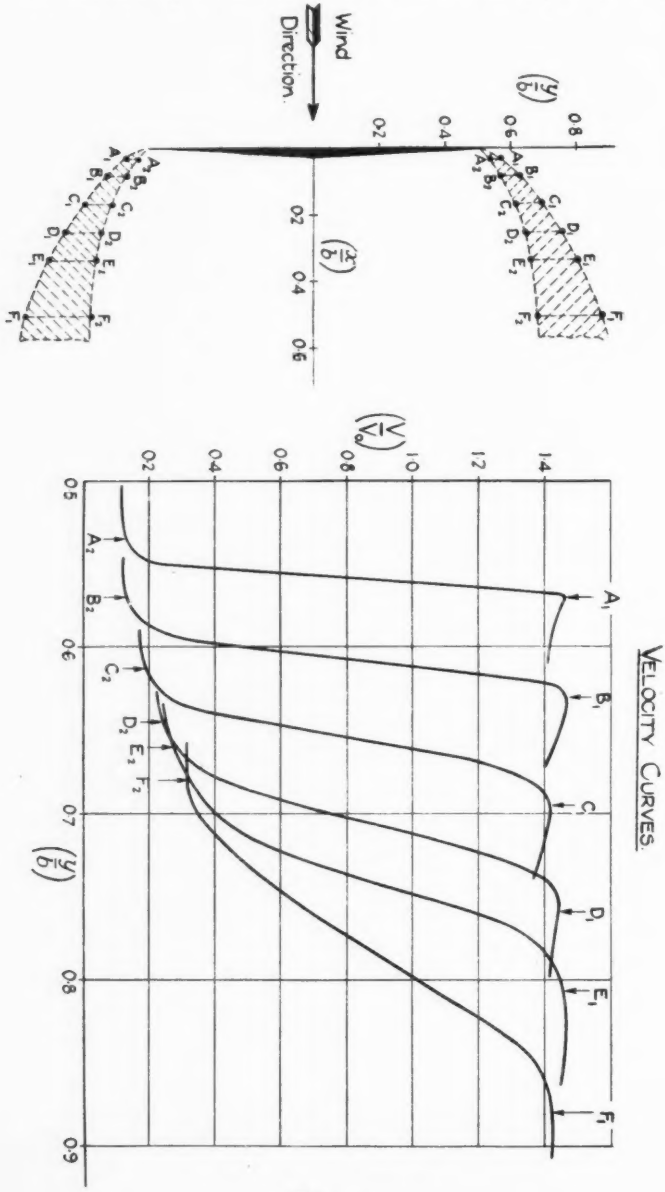



FIG. 7.

that at each section the velocity rises from a small value behind the plate to a maximum value, which is a constant ($V_1/V_0=1.45$) within the accuracy of measurement. The shape of the vortex sheet estimated on the assumption that the outer boundary is the line passing through the points of maximum velocity ($V_1$) and the inner boundary is the line passing through the points of minimum velocity ($V_2$) is also shown in Fig. 7. It will be observed that the breadth of the sheet and also its lateral displacement increase with increasing distance, $x$, behind the plate.

(15) *Vorticity in Sheet.*—If the lines $A_1F_1$ and $A_2F_2$ are the true outer and inner boundaries of the sheets, they will represent the limits of the region of vorticity in the field behind the plate. In addition, if the motion beyond the outer boundaries is irrotational, the total head in this region of the field will be constant and equal to the value ($H_0$) in the undisturbed motion. To determine whether these conditions of flow are realised in practice, explorations of wind direction ($\theta$) and of total head ($H$) were taken along the transverse lines $A_1A_2$, $B_1B_2$, etc., with the specially designed instrument shown in Fig. 6 (right). The principle of direction measurement is due to Bailey,* and depends on the fact that the shielding effect of a cylindrical rod mounted in a wind stream in front of a heated wire is a maximum when the plane passing through the axes of the rod and the wire is in the direction of the local wind. To obtain compactness, the shielding rod was made from a fine tube and used to transmit the pressure at the mouth of the total head tube. Further details of this instrument are given in the reference cited at the end of the paper.†

The value of the vorticity at any point in the field was determined, in non-dimensional units, from the expression,

$$(b/V_0)\,(\partial v/\partial x-\partial u/\partial y)$$

that is,

$$(b/V_0)\,[\partial\,(V\sin\theta)/\partial x-\partial\,(V\cos\theta)/\partial y]$$

since $v=V\sin\theta$ and $u=V\cos\theta$. The values of $\partial\,(V\sin\theta)/\partial x$ and $\partial\,(V\cos\theta)/\partial y$ were estimated graphically from the slopes of tangents drawn to the curves of ($V\sin\theta$) and ($V\cos\theta$) on $x$ and $y$ bases respectively.

Curves showing the distributions of vorticity across the chosen sections of the sheet are given in Fig. 9. On each of these curves are placed lettered arrows to indicate the positions which were predicted from the velocity curves of Fig. 7, of points on the outer and inner boundaries. It will be observed that these arrows define very closely the lateral limits of the vorticity region. The assumption that the boundaries of the vorticity regions (*i.e.*, the sheets) can be predicted from the velocity curves has therefore been justified. An interesting feature exhibited by the curves of Fig. 9 is that the vorticity has its maximum value not at the centre of the sheet, but at a point situated about 60 per cent. of the breadth from the inner boundary.

(16) *Total-Head Observations.*—We shall now refer to the curves of total head which are given in Fig. 8. In the same manner as before, the lettered arrows define the positions of the boundaries of the sheet. It is at once apparent that beyond the outer boundary (points $A_1$, $B_1$, etc.) the total head ($H$) is approximately uniform and equal to the value ($H_0$) measured in the undisturbed stream, so that the motion in this region is irrotational; and also that the total head is approximately constant at the value ($H_0-1.17\,\rho V_0^2$) at points within the inner boundaries (points $A_2$, $B_2$, etc.), where the air is practically motionless. Since the average measured velocity at points in the inner boundary was $0.15\,V_0$, it follows from the measured value for the total head, that the average pressure is ($p_0-0.68\,\rho V_0^2$). This value is in very close agreement with the pressure measured in the stagnant air at the back of the plate ($p_0-0.69\,\rho V_0^2$). It may be concluded then that the evidence obtained from the curves of Figs. 8 and 9 strongly supports

* (19). † (16).

the view that the positions of the boundaries of a vortex sheet can be predicted with good accuracy from observations of velocity. This method has therefore, because of its simplicity, been used generally throughout the paper (see §21).

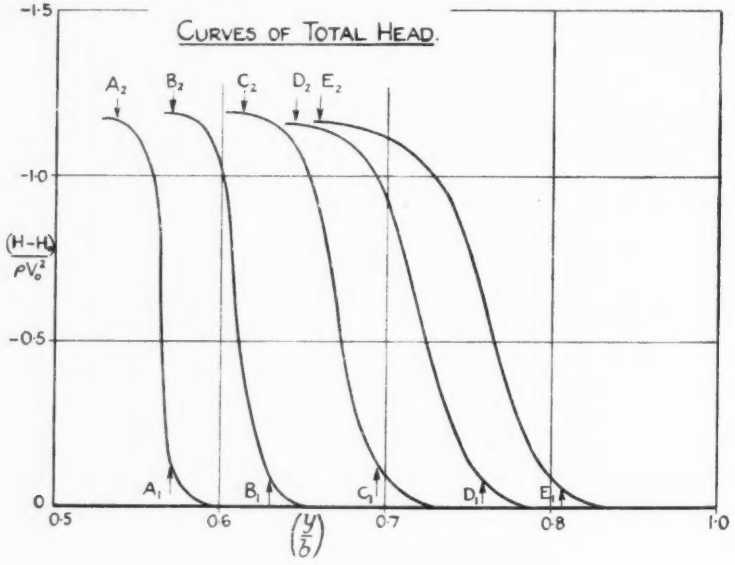



FIG. 8.

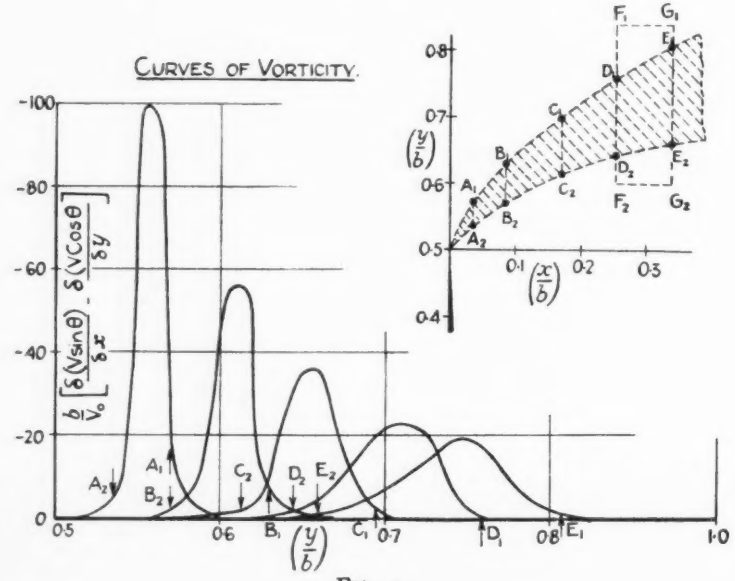



FIG. 9.

(17) *Steadiness in the Sheet.*—The observations of velocity and total head given in Figs. 7 and 8 were taken with instruments which give average values over comparatively long intervals of time, so that these observations may be misleading if the motion were very unsteady. It is necessary, therefore, before these observations can be accepted with confidence, to show that the motion in the region where they were taken is comparatively steady. With this object in view, photographic records of the velocity fluctuations were taken with an Einthoven galvanometer used in conjunction with a hot wire. Three of these records taken at points situated near the outer boundary ($P$), the centre of the sheet ($Q$) and the inner boundary ($R$) respectively are given in Fig. 10a. These records show that at the outer boundary ($P$), where the velocity ($1.42\ V_0$) is a

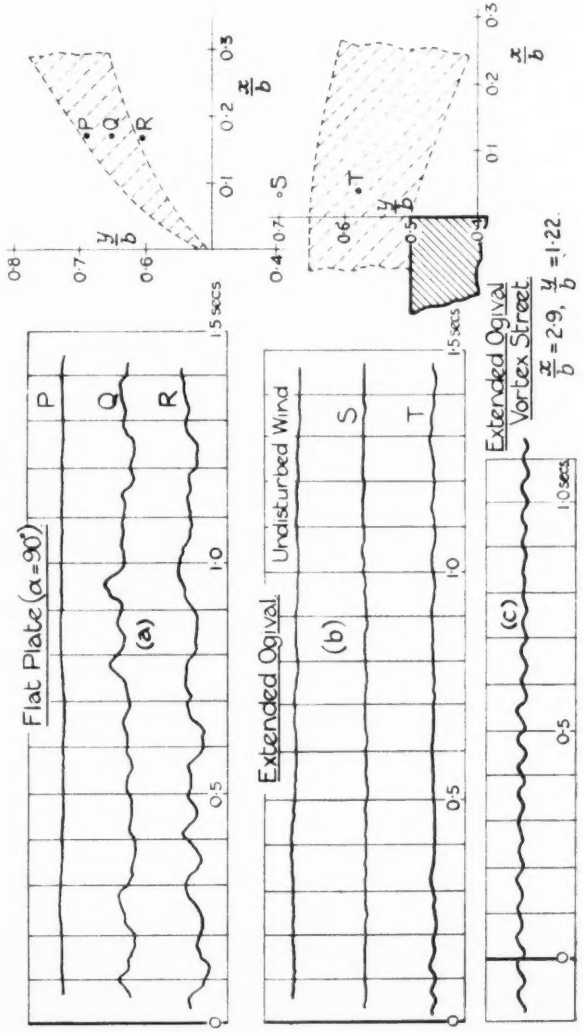



FIG. 10.

maximum the motion is steady. Near the inner boundary ($R$), where the velocity ($0.17\ V_0$) is small, the motion tends to become disturbed; but the fluctuations of velocity do not appear to be regular either in amplitude or frequency—except for occasional slight indications of a frequency which, as will be shown later, is equal to that with which the individual vortices pass downstream. It was also found (these records are not shown) that the velocity fluctuations in the sheet tended to become greater with an increase in the distance downstream, a condition of flow to be expected since the sheets eventually break up into individual vortices. To obtain reliable results, therefore, it was decided to take observations in the region immediately behind an obstacle where, as has just been shown, the motion is comparatively steady. [See also §(27) and records in Fig. 10(b).]

(18) *Flow Across Boundaries.*—To determine these flows, the field containing the sheet was first divided into a series of rectangles by lines taken parallel and normal to the section of the plate. The flow through each arc of the boundary contained between two consecutive transverse lines was then obtained by the evaluation of $\int(V\cos\theta dy - V\sin\theta dx)$ around the contour of that part of the rectangle cut off by the arc. For example, for the arc $D_1E_1$ of the outer boundary (see Fig. 9) the integration was taken around the contour $D_1F_1G_1E_2$, and for the arc $D_2E_2$ around the contour $D_2F_1G_1E_2$. A summary of the results is given in Table 1. It will first be noticed that air is flowing *into* the sheet across *both* boundaries (since the signs of the flows are opposite), and second, that the rate of flow across a unit length of the outer boundary is about 2.2 times greater than that across the inner boundary. The value of this ratio predicted by the Prandtl-Tollmien* theory of the rate of spread of turbulence in a vortex sheet is 2.6.

TABLE 1.

| | Arc (see Fig. 8). | Flow per unit time/$bV$. | Flow per sec. per unit length. | |
|---|---|---|---|---|
| Outer Boundary | $A_1B_1$ | 0.0111 | $0.140\ V_0$ | Mean $0.187\ V_0$ |
| | $B_1C_1$ | 0.0220 | $0.204\ V_0$ | |
| | $C_1D_1$ | 0.0231 | $0.222\ V_0$ | |
| | $D_1E_1$ | 0.0175 | $0.180\ V_0$ | |
| Inner Boundary | $A_2B_2$ | −0.0050 | $-0.081\ V_0$ | Mean $-0.084\ V_0$ |
| | $B_2C_2$ | −0.0059 | $-0.062\ V_0$ | |
| | $C_2D_2$ | −0.0058 | $-0.064\ V_0$ | |
| | $D_2E_2$ | −0.0109 | $-0.128\ V_0$ | |

(19) *Rate at which Vorticity is Shed.*—We have already seen in §15 that the vorticity at any point $(x, y)$ in the sheet is given by

$$[\partial(V\sin\theta)/\partial x - \partial(V\cos\theta)/\partial y]$$

The total amount of vorticity shed from an edge of the plate in unit time—denoted throughout the paper by the symbol $K$—can therefore be expressed in non-dimensional units by

$$(1/V_0^2)\int_{y_2}^{y_1}[\partial(V\sin\theta)/\partial x - \partial(V\cos\theta)/\partial y]\,V\cos\theta dy,$$

where $y_1$ and $y_2$ are the lateral co-ordinates of the points where a transverse line parallel to the plate cuts the outer and inner boundaries respectively. In the case of a sheet of infinitesimal breadth, this expression reduces to $[(V_1^2 - V_2^2)/2V_0^2]$, where $V_1$ and $V_2$ are, as before, the velocities at points in the outer and inner boundaries respectively.

* (20) and (21).

(20) Obviously, an estimation of the value of $K$ from the distribution of vorticity across the sheet is a somewhat laborious matter. It appeared therefore desirable to inquire whether a satisfactory approximation could be obtained from the simple expression $(V_1^2 - V_2^2)/2V_0^2$. Values of $K$ were therefore estimated for several sections of the sheet from the two expressions given above. The comparison is given in Table 2, and shows that a reasonably close estimate of the value of $K$ can be obtained from the expression $(V_1^2 - V_2^2)/2V_0^2$.

Although no definite evidence is available, it is not improbable that closer agreement would be obtained for sheets which have a smaller curvature than that shed from the normal flat plate. The values of $K$ for the vortex sheets considered in §(21) have therefore been predicted from the simple expression by the substitution of the values of the velocities measured at points in the outer and inner boundaries.

TABLE 2.

VALUES OF $K$.

| Section (see Fig. 9). | Estimated from exact expression. | Estimated from approximate expression. |
|---|---|---|
| $A_1A_2$ | 1.18 | 1.07 |
| $B_1B_2$ | 1.20 | 1.07 |
| $C_1C_2$ | 1.15 | 0.99 |
| $D_1D_2$ | 1.11 | 1.02 |
| $E_1E_2$ | 1.10 | 1.05 |

(21) *Sheets Shed from other Obstacles.*—So far, we have restricted our attention to one particular type of sheet, namely, that shed from the sharp edge of a flat plate. It is a matter of common observation that vortex sheets can be shed from bodies without sharp edges, so that we shall now examine, in order to obtain more generality in our investigation, the structure of the sheets shed from obstacles of widely different form. The obstacles selected were a cylinder, three wedge forms, an aerofoil (above the stall), and the flat plate at 40° incidence. It will be observed that these forms can be divided into two groups; first, those which have a symmetrical cross section with the axis of symmetry in the direction of the relative wind (cylinder and wedges, Fig. 11); and second, those either asymmetrical in shape or arranged asymmetrically to the wind (inclined aerofoil and flat plate, Fig. 12). The sheets shed from each obstacle of the first group are arranged symmetrically, whereas those for the second group are asymmetric both in shape and position. No general description of these obstacles is needed, since the sectional shapes are clearly shown in Figs. 11 and 12. It should perhaps be mentioned that the "ogival" section was formed by the arcs of the cubic parabolas $y = \pm (b/2)(1 + x^3/h^3)$ taken from $x = -h$ to $x = 0$.

(22) The boundaries of the sheets shed from each obstacle—determined as in the case of the flat plate from velocity observations taken close behind the obstacle where the motion was comparatively steady—are clearly indicated by dotted lines in Figs. 11 and 12. A discussion of the structure of these sheets is referred to later (§§ 24-27); but before passing on it should be observed that the diagrams of these figures illustrate in a general manner that the shape of the vortex sheet is influenced by the form and attitude of the obstacle.* It was also found that the velocity, $V_1$, measured at points on the outer boundary of each obstacle was fairly uniform so long as the points were taken near the obstacle; and that the velocity, $V_2$, at points on the inner boundary was small and slowly increased with the distance of the point downstream.

(23) The rate at which vorticity is shed from an obstacle was determined for each sheet by substituting average values of $V_1$ and $V_2$ measured near the

* See also (22).

obstacle in the approximate expression $K=(V_1^2-V_2^2)/2V_0^2$. These values of $K$ for the two sheets shed from each asymmetric obstacle (aerofoil and inclined flat plate) are collected in Table 3. The close agreement which is seen to exist between the two series of values given in this table establishes experimentally the

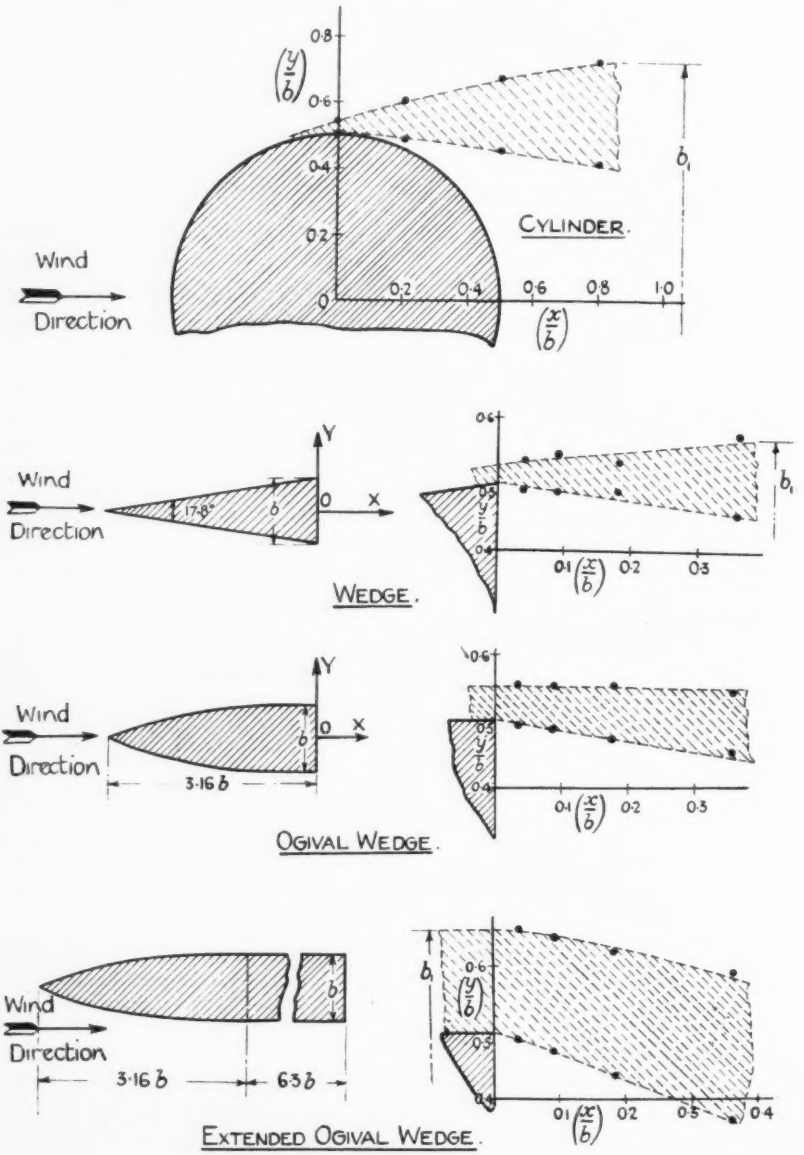



FIG. 11.

important fact that vorticity is shed from the upper and under surfaces of an asymmetric model at the same rate. The values of $K$ for the symmetrical models are given later in Table 7.

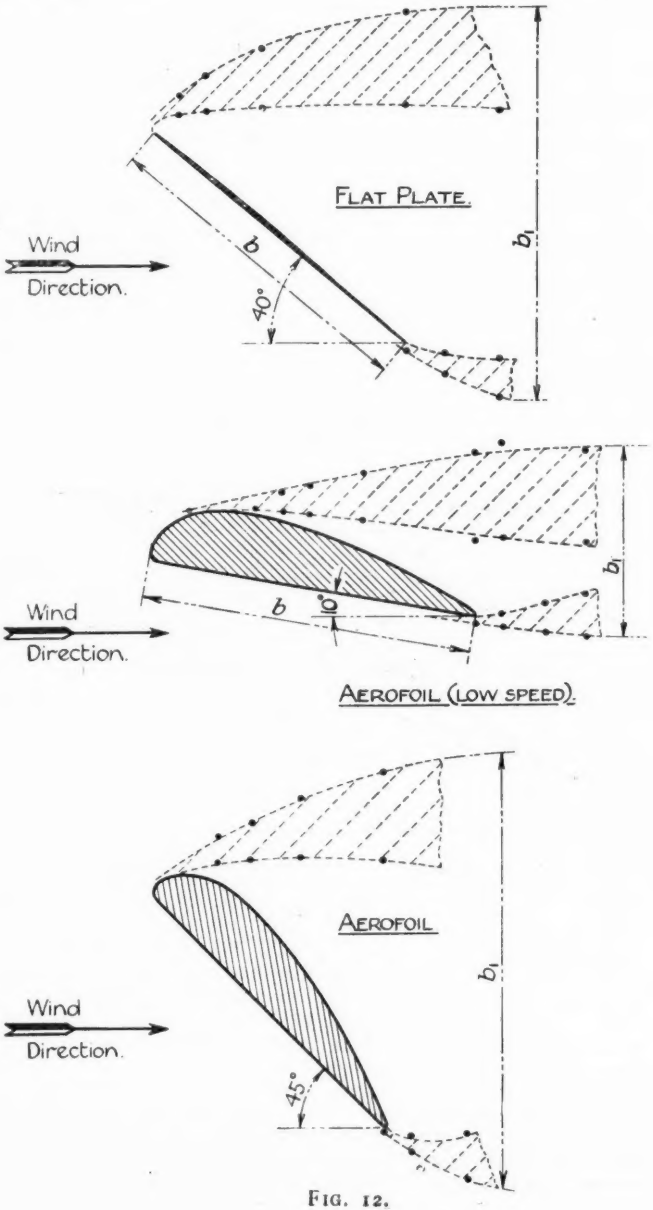



FIG. 12.

TABLE 3.
VALUES OF $K$.

| | Upper surface. | Under surface. |
|---|---|---|
| Aerofoil ($\alpha=10°$) ... ... | 0.70 | 0.69 |
| Aerofoil ($\alpha=45°$) ... ... | 0.94 | 0.93 |
| Flat Plate ($\alpha=40°$) ... ... | 0.93 | 0.92 |

(24) *Velocity Distributions in Sheets.*—Curves showing the distribution of velocity in the sheets shed from each obstacle are given in Fig. 13. Each of these curves has been obtained by drawing a mean curve through a large number of observations taken close behind the obstacle. Also, for the convenience of comparison, the method of presentation adopted is to plot the values of $(V/V_1)$, where $V_1$ is the velocity measured at a point in the outer boundary, on a common base representing the breadth, $b$, of the sheet. On each curve is given the value of $\partial\Delta/\partial s$, that is, the rate at which the breadth, $\Delta$, of a section increases with its distance, $s$, downstream. It will be noticed that the experimental values of $\partial\Delta/\partial s$ for the various sheets lie between the limits 0.15 and 0.36. To standardise the comparison, the theoretical velocity curves (they are identical except near the inner boundary) predicted by the Prandtl-Tollmien* theory for these two values of $\partial\Delta/\partial s$ are also included in Fig. 13.

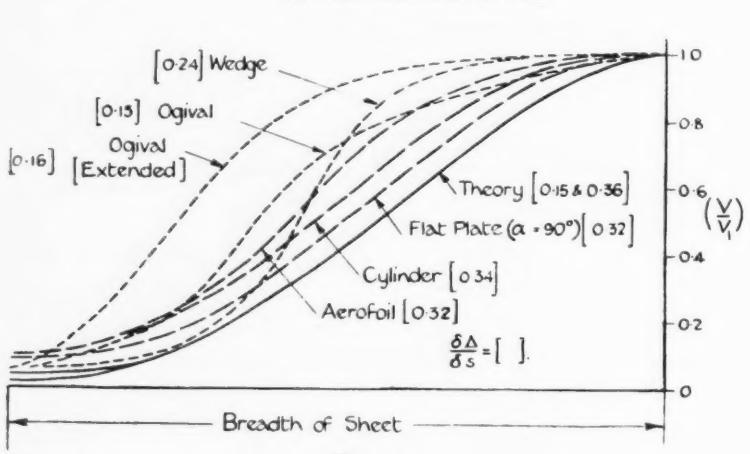



FIG. 13

(25) The most apparent feature exhibited in Fig. 13 is that the experimental velocity curves show a progressive departure from the theoretical curve in the order: Flat plate, cylinder, aerofoil, flat-sided wedge, ogival wedge, extended ogival wedge. Further, it appears that the obstacles can be arranged in two more or less distinct groups: (Flat plate, cylinder, aerofoil) and (wedges). Thus, the velocity curves of the first group resemble more closely the theoretical curve and the values of $\partial\Delta/\partial s$ are in close agreement; whereas those of the second group show more marked departures from theory and the values of $\partial\Delta/\partial s$ are, by comparison, distinctly lower. Another difference—compare Figs. 11 and 12—is that each sheet of the first group has a relatively smaller breadth where it

* (20) and (21).

leaves the obstacle; a phenomenon of the flow probably associated with the fact that the surface exposed to the air stream for each obstacle of the first group is relatively smaller for the same area of obstruction. That this argument is not without foundation would appear to follow from the work on the ogival wedge where the addition of the extension piece increased the overall length in the ratio 3 : 1, and at the same time increased the breadth of the sheet leaving the model by about the same proportion (2.8 : 1). The experiments demonstrate, then, how the shape of an obstacle can influence both the rate of opening-out and the velocity distribution of the vortex sheets trailing behind.

(26) One of the assumptions of the Prandtl-Tollmien theory is that the breadth of a sheet is initially zero. The sheet which most clearly realises this condition is probably that shed from the sharp edge of the flat plate ($\alpha=90°$). It is therefore perhaps significant that the velocity distribution across this sheet should be in closest agreement with that predicted by theory.

(27) In an interesting paper* on the growth of eddies in a viscous fluid, Prof. H. Levy has tentatively suggested that the vortices of appreciable size behind an obstacle may be created by the coalescence of neighbouring vortices as they pass along the surface from nose to tail. We should therefore expect, if this supposition is correct, to find large periodic disturbances in the vortex sheet where it leaves the obstacle. To obtain information on this matter, records of the velocity fluctuations were taken both within and without the vortex sheet shed from the extended ogival wedge, an obstacle selected because it has the largest surface exposed to the air stream. These records are given in Fig. 10*b*, together with a record taken in the undisturbed wind. They show that although there are occasional indications of a slight periodic disturbance, the motion both within and without the sheet is comparatively steady. These records do not therefore support Prof. Levy's suggestion; and in fact throughout the present work there appeared to be fairly definite evidence that the large vortices which pass downstream at some distance behind an obstacle, are created by the rolling-up of the sheets after they have left the obstacle. A record of the velocity fluctuations due to the vortices in the *vortex street*, taken at a point ($x/b=2.9$, $y/b=1.22$) situated a large distance behind the extended ogival model is also included in Fig. 10. A comparison of this record (*c*) with records (*b*) shows that the velocity fluctuations within the sheet are small compared with those outside the street at some distance behind the obstacle.

(28) *Pressure at an Outer Boundary.*—It has been mentioned in §16 that the pressure measured at points on the inner boundary of the sheet shed from the flat plate ($\alpha=90°$) is practically the same as that measured directly in the dead air at the back. It is therefore of interest to observe in Table 4 that the pressure ($p_1$) at points in the outer boundary of the sheet is slightly greater than the pressure ($p_m$) measured at the back; so that it would appear that for the flat plate the pressure across the vortex sheet is not uniform, but that it slightly increases towards the outer boundary. It is also shown in Table 4 that for the other obstacles, the pressure ($p_1$) is, in general, slightly greater than the pressure ($p_m$). The values of $p_m$ given in Table 4 were measured directly; whilst those of $p_1$ were deduced from observations of velocity and total head.

TABLE 4.

$p_o \equiv$ PRESSURE IN UNDISTURBED STREAM.

| Obstacle. | $(p_1-p_o)/\rho V_o^2$ | $(p_m-p_o)/\rho V_o^2$ |
|---|---|---|
| Flat Plate ($\alpha=90°$) ... ... | −0.65 | −0.69 |
| Flat Plate ($\alpha=40°$) ... ... | −0.47 | −0.55 |
| Aerofoil ($\alpha=45°$) ... ... | −0.47 | −0.51 |
| Cylinder ... ... ... | −0.49 | −0.51 |
| Ogival Wedge ... ... ... | −0.183 | −0.252 |
| Extended Ogival Wedge ... | −0.207 | −0.225 |

* (23).

## *Vortex Streets*

(29) We shall now study the vortex street régime at some distance behind the flat plate; and afterwards proceed to consider, in less detail, this type of motion behind the other obstacles.

The problems we set out to answer in our work on the flat plate were: How do the frequency and speed with which the individual vortices pass downstream vary with the incidence of the plate? How are the dimensions of the street influenced by the incidence? And what proportion of the vorticity leaving the edges of the plate travels downstream in the form of well-defined vortices?

(30) *Flat Plate.*—It was thought desirable at the outset to obtain information on the character of the velocity fluctuations at different parts of the field, and

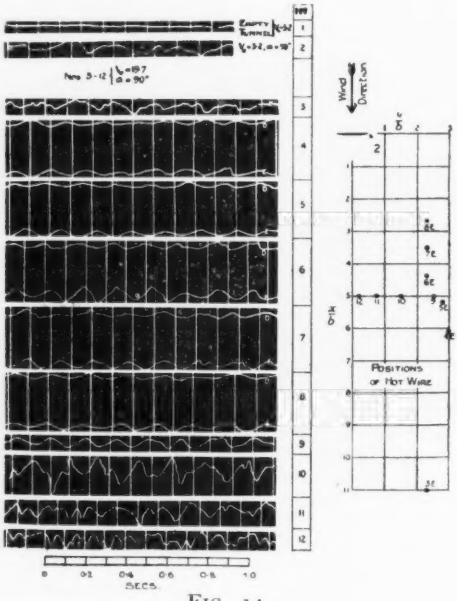



FIG. 14.

also to determine whether the frequency of these fluctuations was uniform. For this purpose the plate was set at 90° to the undisturbed wind, and records of the velocity fluctuations were taken at a large number of points situated both within and without the wake. The method of measurement was to pass the current changes in a hot wire of small diameter, due to the fluctuating air stream, through an Einthoven galvanometer fitted with lantern and camera, and to record photographically the movement of the string of the galvanometer. Included on the records were time spacings of 1/50 sec. obtained from a phonic-wheel time marker, controlled by a 50-cycle tuning fork. The records were taken at a low wind speed in order that any lag in the response of the hot wire—this does not affect the recorded frequency—should not appreciably misrepresent the general character of the velocity fluctuations.

(31) A few of the records taken—and also a diagram giving the positions of the hot wire in relation to the plate—are given in Fig. 14. From these records we learn that the velocity fluctuations at the back of the plate (Nos. 3 to 12) are large compared with those in the empty tunnel (No. 1); that at the

same wind speed, the frequency of the fluctuations at all points behind the plate and at some distance outside the wake is uniform (Nos. 4 to 8); and that the velocity fluctuations within the wake are more irregular in amplitude than those outside and have, in general, double the frequency (compare Nos. 9 and 12).

The most important conclusion to be drawn from these records is that although the amplitude of the velocity disturbances tends to become less regular with the distance downstream, there is a large region situated outside the wake, and at some distance behind the plate, where well-defined fluctuations of uniform frequency can be measured (Nos. 5 to 8). Further, since these fluctuations are caused by the large vortices in the wake, it follows that the frequency with which these vortices pass downstream can best be measured, not in their actual paths, but at some distance outside the wake. When therefore a suitable position for the hot wire is chosen, the Einthoven galvanometer affords a convenient and powerful method of measuring vortex frequency.

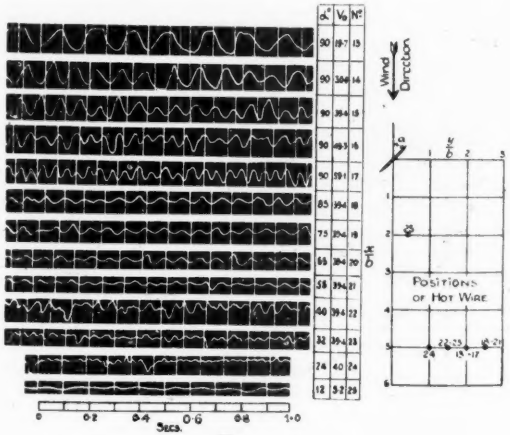



FIG. 15.

It is perhaps desirable to refer at this stage for a moment to Dr. Richardson's lecture* delivered before the Society last year, in which he says (page 843 of the JOURNAL) that his short experience with an Einthoven galvanometer was not very encouraging. The records of Figs. 14 and 15 indicate that a probable reason why Dr. Richardson's efforts were not successful was that he did not select a suitable position for the hot wire.

(32) Records Nos. 13-17 of Fig. 15 show how the vortex frequency, $f$, varies with the wind speed ($V_0$) at a constant angle of incidence (90°). An analysis of records such as these showed that the value of $(fb/V_0)$, where $b$ is the breadth of the plate, has the constant value 0.146 (within 2 per cent.) over an experimental range of wind speed from 10 to 60 ft. per sec. The value of $(fb/V_0)$ measured by Kármán† for a flat plate in water was 0.145.

(33) A large number of records was taken to show the variation of vortex frequency with the inclination $\alpha$ of the plate. A representative selection of these records for values of $\alpha$ between 12° and 19° is given in Fig. 15. They show that in all cases the velocity fluctuations are fairly regular; and that the best diagrams are obtained at high angles of incidence. It is important to observe that velocity fluctuations at the lowest angle of incidence (12°) are much larger and easily distinguishable from the velocity disturbances in the empty

* (24). † (13).

tunnel (Nos. 1 and 25). Values of the vortex frequency estimated from these and similar records, and expressed in the non-dimensional form $(fb/V_0)$, are plotted in Fig. 16. The full line drawn through these points shows that the frequency increases, at first slowly and then more rapidly, as the angle of incidence decreases progressively from 90°.

Values of $(fb \sin \alpha/V_0)$ estimated from the experimental observation were found to be approximately constant and equal to 0.148, for values of $\alpha$ between 90° and 30°; in other words, the vortex frequency for an inclined flat plate is the same as that for a normal plate with the same width of obstruction.

(34) Included in Fig. 16 is a dotted line drawn through crosses which represent observations taken for the inclined aerofoil shown in Fig. 12 (compare Tyler*). This dotted line lies very close to the full line drawn for the flat plate,

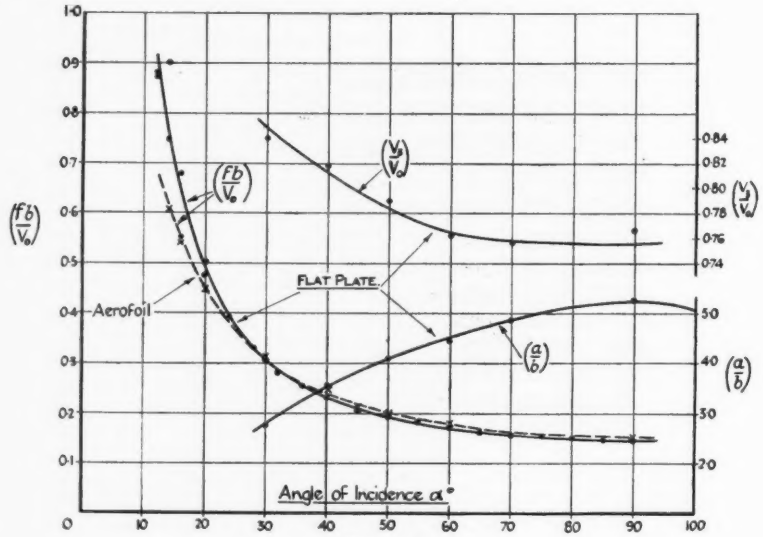



FIG. 16.

except for angles below 25°. The close agreement at a large incidence is not surprising because only the flat under surface of the aerofoil, which resembles a flat plate except for the curvature at the edges, is then presented to the air stream; at small incidences, this is not the case as a part of the upper surface is also exposed.

(35) *Longitudinal Spacing of Vortices.*—The method of measurement is based on the fact that the velocity fluctuations at two points situated without, and on the same side of a vortex street are in phase, when the longitudinal distance between the points is equal to the distance between successive vortices in the same row, or a multiple of this distance. The phase difference between two points was determined from records of the velocity disturbances taken simultaneously with two hot wires, and is equal to the time displacement ($t$) between two series of crests (or troughs) expressed as a fraction of the periodic time ($T$). In practice, one hot wire was fixed whilst the other, usually on the opposite side of the vortex street, occupied several positions on a longitudinal line taken outside the street. Representative diagrams, taken with the plate at

* (25).

90°, are given in Fig. 14 (Nos. 4 to 8). Record No. 4 shows that when the hot wires are situated at the same distance behind the plate, but on the opposite sides of the street, the velocity fluctuations are 180° ($t/T=0.5$) out of phase.

It was found that when the values of ($t/T$) were plotted against the distance downstream ($x$) they fell very closely on a straight line. The longitudinal spacing ($a$) between consecutive vortices in the same row, that is, the change in the value of $x$ when the phase difference is unity, could therefore be directly obtained, in each case from these lines. The estimated values of ($a/b$) for the flat plate are plotted against incidence in Fig. 16. The longitudinal spacing is seen to decrease as the angle of incidence decreases. It was also found, for values of $\alpha$ between 30° and 90°, that ($a/b \sin \alpha$) was approximately constant. It may be of interest to mention that the values of ($a/b$) measured by Kármán* for a normal flat plate immersed in water was 5.50, a value which agrees within 5 per cent. with that measured in the present experiments (5.25).

(36) The speed, $V_3$, with which the vortices pass downstream can obviously be obtained from the product of "$a$" and "$f$," for in unit time "$f$" vortices spaced a distance "$a$" apart are shed from the plate. Values of ($V_3/V_0$) estimated in this manner are also plotted against $\alpha$ in Fig. 16; where it will be observed that the speed $V_3$ progressively increases from 0.76 $V_0$ at $\alpha=90°$ to 0.84 $V_0$ at $\alpha=30°$.

(37) *Lateral Spacing of Vortices.*—The principle underlying the method used to determine the lateral spacing between the vortex rows can perhaps be best illustrated by reference to a hypothetical flow. Let us assume that the vorticity passes downstream in circular columns of diameter "$d$," and let "$h$" be the lateral distance between the two paths traced out by the axes of these columns. Then for two-dimensional motion, the velocity at a point situated outside the street is a maximum when the axis of a vortex in the near row passes across a line drawn through the point normal to the general direction of motion. Also the value of the maximum velocity increases the nearer the point is taken to the street. Hence the points of highest maximum velocity are where the lines $y=\pm(h+d)/2$ touch the circular peripheries of the vorticity areas, where $y$ the lateral co-ordinate is measured from the centre line of the street. Similarly the points of lowest minimum velocity are where the lines $y=\pm(h-d)/2$ touch the peripheries. The diameter "$d$" can therefore be regarded as the lateral distance between the points of highest maximum and lowest minimum velocity on the same side of the centre line, and the lateral spacing "$h$" as twice the mean of the lateral distances of these points from the centre line of the street. The values of "$h$" and "$d$" for the wind-tunnel flows were therefore estimated on this analogy from the positions of the points in the field where the highest maximum and lowest minimum velocities occur.

(38) The curves from which the lateral positions of the points of highest maximum velocity and lowest minimum velocity were deduced, were estimated indirectly from observations of mean velocity and records of velocity fluctuations taken both within and outside the vortex street at several sections behind the plate, namely, $x=5b$, $10b$ and $20b$ for $\alpha=90°$ and $8b$ for $\alpha=70°$ and $\alpha=40°$. These curves of maximum and minimum velocity could have been constructed from observations taken directly with the Einthoven galvanometer, but this was not done because, as will be shown later, the actual magnitudes of the velocity fluctuations were needed for the determination of the strength of the individual vortices.

To minimise any lag in the response of the hot wire to the velocity fluctuations, the Einthoven records were taken at a low wind speed (about 10 ft. per sec.). A comparison of the records taken at speeds above and below 10 ft. per sec. showed that if there is any error due to lag it is smaller than irregularities in the velocity fluctuations which normally occur at any point; and in fact the

* (13).

records showed, except at a point well outside the limits of the street, that there were large variations with time, in the values of both the maximum and minimum velocities. Average values of the velocity fluctuations were therefore estimated from the average displacements between a large number of crests and troughs.

(39) A set of curves, which show the variations of the maximum, mean, and minimum velocities along a transverse line situated at a distance $10b$ behind the plate at $\alpha=90°$, is given in Fig. 17. These curves are representative of those taken at other sections. On passing through the wake from the outside, the mean velocity drops slowly at first and then more rapidly until a minimum value is reached at the centre of the street. The amplitude of the velocity fluctuations, that is, the difference between the maximum and minimum velocities, is small at points well outside the wake but rapidly increases as the street is approached. It is clearly shown that the maximum velocity at a point outside the wake increases as the point is taken nearer the street and eventually reaches a well-defined maximum (marked $M$ in Fig. 17). It follows then from the foregoing discussion that the lateral position of the outer boundary of the street is also well defined. The point of lowest minimum velocity, and so the lateral position of the inner boundary, is not however so well defined, but appears to be situated somewhere near the centre of the wake ($y=0$). It follows then from the discussion given in §(37) that at this section (and also at the other sections chosen) the diameter ($d$) of the vorticity area is quite large and of the same order of magnitude as the lateral spacing ($h$) between the vortex rows.

(40) The values of $(h/b)$, for various values of $(x/b)$ and $\alpha$—estimated on the assumption that the vorticity areas extend to the centre line of the street and that their diameter and lateral spacing are equal—are given in Table 5.

TABLE 5.

| | Wind Tunnel. | | | | |
|---|---|---|---|---|---|
| $\alpha°$ | $(x/b)$ | $(h/b)$ | $(y/b)$ co-ord. of point in outer boundary. | $(a/b)$ | Kármán $(h/b)$ |
| 90 | 5 | 1·3 | 1·3 | 5·25 | 1·48 |
| | 10 | 2·0 | 2·0 | | |
| | 20 | 2·75 | 2·75 | | |
| 70 | 8 | 1·9 | 1·9 | 4·85 | 1·36 |
| 40 | 8 | 1·3 | 1·3 | 3·55 | 1·00 |

Included in this table are the theoretical values of $h/b$ obtained when the experimental values of "$a$" are substituted in Kármán's stability relation* $h=0.281a$. The wind-tunnel values of "$h$" are observed to be greater—except at $\alpha=90°$; $x/b=5$—than those estimated by the Kármán theory. It should be noted, that if the vortices extend beyond the centre line of the wake the wind-tunnel values of $(h/b)$ will be over-estimated. The differences between the experimental and theoretical values of $(h/b)$ in Table 5 may therefore be partly due to the impossibility of locating with any certainty, from curves such as those in Fig. 17, the position of the inner boundaries, especially if the vorticity areas extend beyond the centre line. On the other hand, there appears to be definite evidence, from the values of $(h/b)$ taken at $\alpha=90°$, that there is in contradistinction to theory, an appreciable opening-out of the wake with increasing distance downstream. Further, this opening-out can be accounted for only by separate or joint increases in "$h$" and "$d$." In the above method of estimation we have assumed that both "$h$" and "$d$" increase at the same rate.

(41) *Vorticity in the Wake.*—The relation between the strength ($\kappa$) of each

* (26).

individual vortex of an infinite street and the velocity fluctuations $(V_{max} - V_{min})$ at a point distant $y$ from the centre line is given by the expression

$$V_{max} - V_{min} = (\kappa/a) \{ 1/[\sin h \cdot 2\pi (y - h/2)/a] + 1/[\sin h \cdot 2\pi (y + h/2)/a] \}$$

where "$a$" and "$h$" are the longitudinal and lateral spacings respectively of the vortices. To determine then the value of $\kappa$ when the values of "$a$" and "$b$" are known, it is only necessary to measure the magnitude of the velocity fluctuations $(V_{max} - V_{min})$ at several points in the field, and preferably at some distance outside the wake where the fluctuations have a comparatively uniform amplitude (see Fig. 14). To determine whether the above formula, which was derived on the assumption that the length of the street was infinite, was applicable to the street of finite length, a second formula was obtained which gave the relation between $\kappa$ and $(V_{max} - V_{min})$ for the actual spacing of the vortices in the wind tunnel, when the plate was at 90°. These two formulæ were found to give results in close agreement so that the above formula, which was preferred because of its simplicity, was used to determine the values of $\kappa$ given in Table 6.

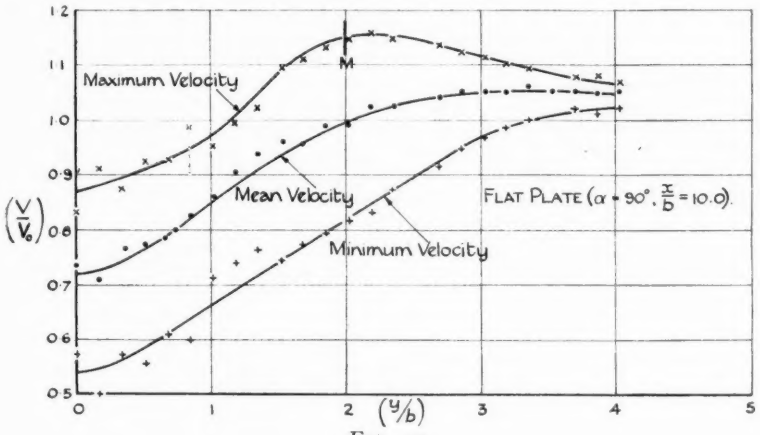



FIG. 17.

(42) Estimations of the average strength of the individual vortices, at a distance of about $9b$ behind the plate, were made for incidences of 90°, 70° and 40° respectively. The values of vortex strength were determined for both the wind-tunnel and Kármán values of "$h$," since the former were known to be somewhat uncertain; and also for two values of $(V_{max} - V_{min})$ measured at different points in the same lateral line. The values of the vortex strength, $\kappa$, have been expressed in non-dimensional units by dividing by the product $(V_0 b)$. It will be observed in Table 6 that the two series of values of $(\kappa/V_0 b)$ calculated for the two values of $(V_{max} - V_{min})/V_0$, at the same value of $(h/b)$, are, in general, in close agreement; and also that the values of $(\kappa/V_0 b)$ are smaller in Series II. than in Series I. The ratios of the rate at which vorticity passes downstream in the form of vortices having a definite individuality to the rate at which vorticity is shed from the edge $(\kappa f/K)$, are given in the last column of Table 6. These results show that at a distance of $9b$ (approximately) behind the plate about 60 per cent. of the vorticity leaving the edge passes downstream as large discrete vortices. Whilst no positive evidence is available it is probable that a part of the rest is dissipated close behind the plate, by a mixture of the positive and negative vorticity from the two edges, and that the rest possibly passes downstream as "unattached" vortices which are too small to affect appreciably the

velocity fluctuations measured outside the wake. We must also remember that the large vortices themselves tend to break up and so to lose their identity, as their distance behind the plate increases.

TABLE 6.

| | | | Series I. Kármán. | | | Series II. Wind Tunnel. | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| $\alpha°$ | $(y/b)$ | $(V_{max.}-V_{min.}/V_c)$ | $(h/b)$ | $(\kappa/V_c b)$ | $(\kappa f/V_c^2)$ | $(h/b$ | $(\kappa/V_c b)$ | $(\kappa f/V_c^2)$ | $(K/V_c^2 i$ | Ser. I. (Mean) | $(\kappa f/K)$ Ser. II. (Mean) |
| 90 | 3.0 | 0.151 | 1.475 | 5.1 | 0.74 | 2.0 | 3.9 | 0.57 | 1.06 | 0.68 | 0.53 |
| | 3.8 | 0.055 | 1.475 | 4.8 | 0.70 | 2.0 | 3.8 | 0.55 | | | |
| 70 | 3.0 | 0.128 | 1.365 | 5.3 | 0.83 | 1.9 | 4.1 | 0.64 | 1.00 | 0.82 | 0.63 |
| | 4.0 | 0.034 | 1.365 | 5.2 | 0.81 | 1.9 | 4.0 | 0.62 | | | |
| 40 | 1.8 | 0.146 | 1.000 | 2.2 | 0.51 | 1.3 | 1.8 | 0.41 | 0.93 | 0.64 | 0.51 |
| | 2.5 | 0.057 | 1.000 | 3.0 | 0.69 | 1.3 | 2.4 | 0.55 | | | |

(43) *Vortex Streets behind other Obstacles.*—To determine some general characteristics of vortex streets, measurements were made for each of the obstacles shown in Figs. 11 and 12 of the frequency, $f$, and the speed, $V_3$, with which the vortices pass downstream, and also of the longitudinal spacing ($a$) between consecutive vortices in the same row. These results are collected in Table 7. No comment on these individual values is needed except perhaps to mention that the value $(fb/V_0)$ for the cylinder was found to be in agreement with that measured at the same $(vl/\nu)$ by Relf* and Simmons, who used the vibration galvanometer method. Included in Table 7 are the values of the velocity $(V_1)$ at points in the outer boundary of the sheets, and also of the total strength of vorticity, $K$, passing a cross section of a sheet in unit time. A comparison of the values given in columns 1, 2 and 5 shows that, in general,

TABLE 7.

| Obstacle. | $(V_1/V_0)$ Col. 1. | $K$ Col. 2. | $(fb/V_0)$ Col. 3. | $(a/b)$ Col. 4. | $V_3/V_0$ Col. 5. | $(b^1/b)$ Col. 6. | $(fb^1/V_0)$ Col. 7. | $(a/b^1)$ Col. 8. |
|---|---|---|---|---|---|---|---|---|
| Flat plate ($\alpha=90°$) | 1.45 | 1.06 | 0.146 | 5.25 | 0.77 | 1.85 | 0.270 | 2.84 |
| Flat plate ($\alpha=40°$) | 1.37 | 0.93 | 0.231 | 3.55 | 0.82 | 1.20 | 0.277 | 2.96 |
| Cylinder ... | 1.39 | 0.96 | 0.187 | 4.27 | 0.80 | 1.45 | 0.271 | 2.95 |
| Aerofoil ($\alpha=45°$) | 1.37 | 0.94 | 0.210 | 3.67 | 0.77 | 1.31 | 0.275 | 2.80 |
| Aerofoil ($\alpha=10°$) | 1.19 | 0.70 | 0.735 | 1.25 | 0.92 | 0.57 | 0.418 | 2.19 |
| Wedge ... ... | 1.32 | 0.86 | 0.238 | 3.45 | 0.82 | 1.15 | 0.273 | 3.00 |
| Ogival Wedge... | 1.15 | 0.66 | 0.271 | 3.16 | 0.86 | 1.10 | 0.298 | 2.87 |
| Extended Ogival Wedge ... | 1.18 | 0.69 | 0.254 | 3.33† | 0.81 | 1.31 | 0.332 | 2.54 |

the speed $V_3$ decreases with an increase in $V_1$ and consequently in $K$. The values of $(fb/V_0)$ and $(a/b)$ for the different obstacles are seen to differ considerably amongst themselves. This is, of course, not surprising since the values of $b$ refer to a particular dimension of each model and so are unrelated. If we assume that the sheets shed from the obstacles were parallel bands of small breadth, and that they had the same velocity $V_1$ in the outer boundary, then the frequency, $f$, would vary inversely, and the longitudinal spacing, $a$, directly as the distance between the bands, at the same velocity $V_0$. Further, let $b'$ represent the distance between the outer boundaries of the sheets, then both $(fb'/V_0)$ and $(a/b')$ have constant values even although the values of $(fb/V_0)$ and $(a/b)$ vary from obstacle to obstacle. If, therefore, instead of $b$ we introduce a new parameter $b'$ (see Figs. 11 and 12) to represent the maximum distance between

* (27). † Value steadily increases with distance behind obstacle.

the outer boundaries of the sheets shed from each obstacle—since the sheets are not parallel and not infinitesimally narrow this appears the best parameter possible—we should expect the frequency coefficients and also the longitudinal spacing coefficients to be in close agreement. Columns 7 and 8 show that close agreement does obtain between the values of $(fb'/V_0)$ and also between the values $(a/b')$, except for the extreme cases of the aerofoil at 10° incidence and the extended ogival wedge.

It would appear then that, at a constant value of $V_0$, both the frequency and longitudinal spacing of the discrete vortices behind a high resistance obstacle depend largely on the lateral spacing between the vortex sheets, and that these characteristics are influenced to a much smaller extent (except possibly in extreme cases) by the shape and structure of the sheets.

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* The subject matter of the lecture has been taken from these papers.

## Discussion

Professor Bairstow (Past Chairman of the Society) said the author had described a number of difficult and unusual experiments, involving a development of wind-channel technique which was in itself a great asset. Probably most of those present would have been impressed by the lack of precise explanation which could be given to the phenomena observed. That, of course, was not Mr. Fage's fault; it was due simply to our lack of knowledge. Mr. Fage, with a number of other people—many at the National Physical Laboratory and at the Royal Aircraft Establishment at Farnborough—formed a link between the great body of people who were applying existing knowledge to practice and the very limited number who were looking many years ahead, perhaps very many years ahead. He supposed that normally one would speak of these two bodies of people as being, on the one hand, those who practised, and, on the other hand, those who theorised. Theory had rather a bad name, but the word was valuable when restricted to something which had been proved, and when Professor Prandtl gave us a theory for correcting some of our aerofoil results, it was accepted very widely and quickly. There was little doubt that good working theories were wanted. Those engaged in trying to provide material for aircraft design realised that the whole of their working knowledge up to the present, with the exception of the Prandtl theory, was empirical—obtained largely in the wind-tunnel, supplemented by full-scale experiment. The bulk of material had become very great. There was a great deal of it in the reports of the Aeronautical Research Committee, and in the publications of the American National Advisory Committee, and although Britain and the U.S.A. between them did most of the publishing of the world's aeronautical literature, there was an appreciable amount of other work available. It seemed possible that we were reaching the limit of what could be done in the way of direct cataloguing of generally useful information and the evolution of some theory which would connect the bulk of this work together and so reduce the bulk of the experiments required seemed to be necessary. The work of theorising, he considered, in the first place, belonged to the universities; secondly, to the research establishments of the Government, for it was only with adequate experimental facilities that such experiments as Mr. Fage had described could be conducted and so provide a means of checking theories when obtained. Apart from the empirical work which he (Prof. Bairstow) had mentioned, we had only the Prandtl theory of aerofoils and the Kutta-Joukowski theory, which latter, as a matter of fact, was little used in practice. The theory of an inviscid flow as applied to an aerofoil was mathematical and instead of the experiment, in the tank, which Mr. Fage had made, we could do the corresponding work rather more accurately in some respects by calculation in the office. We had, however, no mathematical criterion as to when the theory broke down. We knew in practice that unless an aerofoil had a high ratio of lift to drag we must not apply the Prandtl or the Kutta-Joukowski theories. When somebody like Flettner wanted to make rotating cylinders for driving ships he did not know what lift he would get out of those rotating cylinders, but had to go to the Prandtl laboratory and make experiments. A straightforward application of the Kutta-Joukowski theory leads to an overestimate of the lift of an aerofoil by 25 per cent., or thereabouts. This was the discrepancy which led Prandtl to use wind tunnel results instead of the theory when finding the circulation round an aerofoil. A second discrepancy occurred in the failure to predict the stalling of the aerofoil at all, and Mr. Fage, in his paper, had mentioned a third failure, *i.e.*, the failure to predict the point at which the maximum positive pressure on the aerofoil occurred. That was a secondary matter, perhaps, but it was of some importance. One other theory, mentioned in the second part of the paper, was Kármán's theory of the vortex street, and that, again, gave an approximation of unknown accuracy. There was still another theory—which was perhaps outside the strict range of Mr. Fage's paper—and that was the

theory of dynamical similarity. Professor Jones, of Cambridge, working with Mr. Farren, had devised a new piece of apparatus, *i.e.*, a water tank, in which an aerofoil could be moved through water at high speeds—5ft. per second—whilst photographs were taken of the motion of the water around the aerofoil. The theory which told us that the experiment was good, *i.e.*, the theory of dynamical similarity, was well established, and none of us doubted that we could believe that the results when obtained could be applied to motion through air. Results were only just coming through, but when the experimental knowledge had been extended a little further we should be able to see, under full-scale conditions, just how the fluid moved around an aerofoil. Here, again, was an experiment which was wanted as the basis of a theory. With the direct help of Mr. Farren, a similar tank was being made at the Imperial College, where we had been working for some years on motion of air in the boundary layer of the particular aerofoil to which some of Mr. Fage's experiments related. The difficulties were very great, and, although they had one complete solution of an approximate theory, it was still far from the truth.

Another line of progress is possible which has no reference to experimental work. Mathematical equations of fluid motion were put forward by Stokes some 70 or 80 years ago. The more we looked at those equations, the more did we believe that they contained the statement of all our problems. The difficulties of solution were mathematical; and it seemed to him to be very important that somebody should see what could be done in the direction of solving them. The resistance of small round wires could be calculated from these equations, and so on, but for practical aeronautical purposes we knew very little. In between the extremes—practice, on the one hand, and mathematical theory on the other—a link is valuable, and we welcomed such work as Mr. Fage described for its value as a test of tentative theories which might be used whilst a more fundamental basis was being developed.

Major Low said that so well balanced a paper offered little scope for his New Year's resolution to criticise with rigour at the Society's meetings. A question had long been in his mind and often expressed as to what they were really measuring. In a fluctuating wind, the pitot tube certainly failed, and in the past the hot wire instrument had seemed even less reliable, requiring constant recalibration, and, one might reasonably have thought, requiring different calibrations for different degrees of fluctuation. But from the consistency of the results with each other and with our partial knowledge of the hydrodynamics of the phenomena he concluded that Mr. Relf and his colleagues had so developed the technique of the hot wire instrument as to set up an entirely new standard in exploring the velocity field. They had previously established the existence of circulation round a lifting wing and of vortices trailing from the tips by direct measurement, whereas Prandtl had done so by indirect experimental methods. He thought that this work should rank as classical in the development of aerodynamics. The endless variety of fluid motion would no doubt require a very wide range of further experiment, but Mr. Fage had given them a very good instalment. He had made use of the partial theories available and his results checked them and offered ideas for further developments of physical theory. The lucid commentary was rightly kept within this scope.

In contrast with this intentionally limited range of practicable progress, Professor Bairstow had again advocated more intensive work on the general solution of the differential equations of viscous fluid motion, which should be carried out in our Universities.

His (Major Low's) own contribution to that cause had been to interest Professor Filon, who had recently given post-graduate lectures at University College, London.

Among others the problem of two-dimensional viscous fluid flow past a

cylinder was considered. To make the analysis tractable a contour was taken so large that all hollow-coned vortices had collapsed and all eddying motion had substantially died down. Under these restrictions no information was obtained as to what happened at the boundary nor as to the relations between its size, shape and orientation and the flow produced. The differential equations of the simplest type of cavitation were formed, but were non-linear and highly intractable. It was shown, however, that the Kutta-Joukowsky theorem was still valid. Interesting drag relations were also found, including the equality of drag to rate of increase of momentum in the "tail," as required by the conservation of linear momentum.

The much harder problem of finding the moment was also discussed, and a new type of differential equation was found defining new functions which required tabulation. From the analysis it appeared that Iseen's equations could not be used for further approximations. This result was so unexpected and indeed subversive that the analysis had been checked in detail by members of the class and cross checked in various ways by Professor Filon himself, always with the same result, which could therefore be accepted with some confidence.

Such very great difficulties in a problem so externally simple and so severely restricted, showed that only fully equipped mathematical physicists could be expected to make any progress along the lines advocated by Professor Bairstow.

In the meantime experimental work such as Mr. Fage had described should be continued with the caution that some sort of physical principle should underlie every scheme of measurement and that they should not rest content until the empirical relations derived from their measurements were co-ordinated by some sort of rational physical theory.

They would probably advance faster and further by such alternate steps than by frontal attack on Stokes' equations.

On minor matters he did not like the term "stagnation point" to describe the point at which the effective head was greatest, and at which the fluid was on the point of speeding up again to its maximum velocity. ("Stationary point" or simply "station" might do. He had experimented with "branch point" which had advantages from the mathematical point of view.) The German term "stan-punkt" might be translated as "dam point."

He asked if the areas in Fig. 9 were equal as was to be expected from the diffusion of vorticity.

The departure from symmetry in Fig. 13 might be due to the mutual interference of the upper and lower belts, increasing with their breadth.

It seemed more descriptive to say that the tail diffused into the surrounding fluid than that the fluid flowed into the tail.

The irregularity behind the wedge in Fig. 11 seemed to indicate an undulating instability preceding break up into discrete vortices which in time would come into agreement with Professor Levy's suggestion.

He had had great profit and enjoyment from the paper.

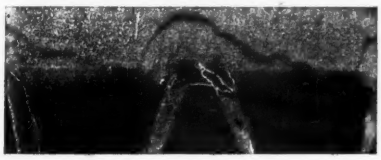
Dr. PIERCY said that when one considered the extent of the vorticity round the aerofoil, it seemed remarkable at first sight that the Kutta-Joukowski theory held so well, and he hoped that the lecturer would have more to say on this point.

Without anticipating some work which was shortly to be published, with Dr. Richardson's help, from East London College, he would say that by a suitable development of hot wire methods they had been able to go rather closer perhaps than usual to the surface, and had found conditions that seemed fairly clearly reflected in Mr. Fage's results. Actually, the flow quite near to the nose was devoid of the vortex sheets that had been pointed out to-night, but their presence over the rest of the upper surface of the aerofoil at any ordinary angle of incidence was an effective demonstration of the fact that the air flow by no means followed that contour. An interesting aspect of the presence of the vortex sheet near the upper surface was that the velocity gradient only became great some distance

from the surface, being small near it—except at the nose. The skin friction near the nose was consequently great, but afterwards fell away rapidly. The vortex sheet permitted a thin boundary layer to exist without large skin friction.

Dr. RICHARDSON dealt first with the shedding of the vortices. Mr. Fage had given a picture (Fig. 12) showing that even at 10° incidence there was a large amount of vorticity shed from the upper as well as from the lower surface. Mr. Tyler, working in his wind channel, had obtained results which confirmed this, even at 0° incidence. He (Dr. Richardson) would like to know, therefore, what was Mr. Fage's view of Prandtl's theory on this matter. Dealing with the rotating cylinder, Prandtl (*Naturwissenschaften*, p. 101, 1925) says, "With sufficiently strong rotation [of the cylinder] there is no retardation and consequently no vortex on the side running with the stream; on the side running against the stream however a vortex is formed—with weaker rotation of the cylinder the vortex becomes smaller, the other larger than with the stationary cylinder." Prandtl apparently applies the same idea of only *one* surface of discontinuity to the aerofoil, the vortices being shed near the trailing edge only. Both Prandtl (*Wiss. Geo. f. Luftfahrt*, 1920) and Bjerknes (*Journ. de Physique*, 1924) suggest that a circulation is set up equivalent and opposite to the vortex shed from the under surface and base a theory of the origin of the lift on this assumption. Would not this idea have to be modified, in practice, in view of the evidence of Mr. Fage and Mr. Tyler that vorticity is shed early along the upper surface, as well as from the trailing edge, even below the stalling angle? Mr. Tyler's results also confirmed the widening out of the avenue behind the obstacle, observed by the lecturer; the trails of the vortices could still be traced some six chords behind the obstacle.

One point on which the speaker disagreed with Mr. Fage was in regard to the lag in the hot wire. Mr. Fage had said that he found the values of the maximum and minimum velocities in the fluctuations—apparently from the string galvanometer record. If he understood that correctly, Mr. Fage had calculated directly from the maximum and minimum of the oscillation record—say, from a static calibration curve. There had been a great deal of debate as to whether the lag could be neglected in the hot wire used in that way, and he mentioned three researches on the subject. In the first place, Mr. R. C. Richards, at Cambridge, had oscillated a hot wire on pendulums and vibrating tuning forks, in which the magnitude of the velocity fluctuations was known *a priori*, and had found that only a fraction of this fluctuation was recorded by the hot wire, that fraction becoming less as the frequency of the oscillation and the diameter of the wire increased. Secondly, he himself had oscillated the hot wire on a cam worked by an engine. The annexed figure shows one of the results; the thick line is the



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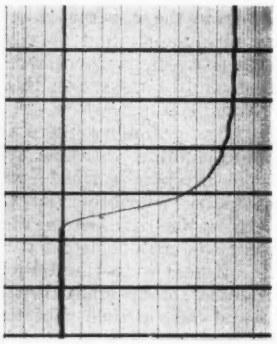
self-recorded movement of the cam; the dark line the record of the galvanometer. The lag in recovery here is very marked, so that although the motion was a symmetrical one, the lag in recovery makes the hot wire record asymmetric (the frequency was 10 per second). The third research, by Dr. Anrep and others, was carried out in the Institute of Physiology at University College. They had wanted an anemometer to record fluctuations in airflow for application to the

blood system. They stated definitely that a correction is necessary if absolute values of the fluctuations are required. This figure shows the response of the galvanometer to a sudden change from zero to a definite velocity. The record ought, of course, to show two right-angle bends, but the lag is very apparent.



(By kind permission of *The Heart*.)

For this and other reasons, the speaker had abandoned such a method for studying rapidly fluctuating airflow, and had developed the "step-by-step" anemometer (in which the lag was allowed for in calibrating) as previously described to the Society.

He asked what diameter of wire was used by Mr. Fage, and whether he had made any test as to the magnitude of the lag. If this were known he thought that a correction might be applied to Fig. 17, if necessary, and suggested that the deduction that 60 per cent. of the vorticity passes downstream as discrete vortices might need modification, when the lag was allowed for. The lag would not affect the value of the *frequency*, and at a given frequency there would always be a *proportionality* between the magnitude of the actual fluctuations and that deduced from the galvanometer record.

In conclusion he would like to congratulate Mr. Fage on an interesting paper, and particularly on the beautifully regular records shown by him in Figs. 14 and 15.

Mr. GLAUERT suggested that, as the aerofoils used were very thick and, according to the curve, the drag was very high—two or three times what one would expect from a good shape—the results would have been more satisfactory with an aerofoil of better shape. He had been very much interested in the vortex street behind the flat plate. We had seen that the vorticity areas extended to the centre of Mr. Fage's figure. If those areas of vorticity did in fact cross the centre line, as Mr. Fage had suggested—and it certainly seemed probable—there must be a certain amount of mingling of the vortices of the upper row with those of the lower row, and it seemed to him that that explained two facts; first, that the breadth of the street, as obtained by the experiments, did not agree with Kármán's theory, and secondly, that the street widened out, and appeared wider than it should be according to Kármán's theory; that process continued as one passed down stream. When we obtained accurate numerical values and tried to compare them with Kármán's theory we met the problem as to how far the flow had been influenced by the channel walls. He had tried to extend the theory to cover the case of a vortex street in a channel, and undoubtedly there was an influence, for which allowance must be made before it could be said that the theory was inaccurate, or to what extent it was inaccurate. The plate, as tested by Mr. Fage, had 15 per cent. higher drag than it would have had if the channel

walls had been absent, and that, of course, must re-act on the characteristics of the vortex street behind the body. He could not agree with the remarks of Dr. Richardson and doubted very much that Mr. Fage would agree; he believed Mr. Fage would be very much surprised to find that his experiments were contradictory to Prandtl's theory as to how an aerofoil obtained its lift. The aerofoil started from rest, a vortex was shed at one stage, and a corresponding circulation came on the aerofoil, but that had nothing whatever to do with Mr. Fage's experiments. Mr. Fage had said simply that in the later stage, when the motion had become steady, there was as big a vortex from the upper surface as from the lower, and the fact that he had got that result was not something he was proving, but it was a check on the accuracy of his experiments, because if he had found that vorticity were systematically being shed at a greater rate from one surface than the other, then the circulation round the body would be steadily increasing or decreasing, and that would mean that the lift of the aerofoil was increasing steadily with time. Again, Mr. Glauert also considered that Dr. Richardson was off the line when he had suggested that the possible inaccuracies in the method of measurement would account for the 40 per cent. loss of vorticity which Mr. Fage had found in the vortex street as compared with what was shed from the edge. The amount that was shed from the edge was determined simply by the velocity outside the vortex sheet, and was determined in two or three independent ways, so that it was quite a reliable determination, and he believed he was right in saying that it was quite independent of the hot wire instrument at all. The amount that came down in the vortex street was a more complex matter to determine, and they might possibly quibble as to the exact value, but if one inserted that value into Kármán's formulæ for the drag of the flat plate approximately the correct value was obtained, whereas if one inserted the full value of the vortex sheet, the drag ought to be approximately double. The fact that the vorticity which came off the edge did disappear in that way was pointed out by Prandtl several years ago. The vortex sheets came off from the edge, became unstable, and broke up. Was there any particular reason for assuming that, in that process of breaking up, all the vorticity of this vortex band should appear in the distinct vortices which eventually passed down the stream? There was a region of considerable turbulence, and in this general turmoil half the vorticity disappeared. It seemed quite a reasonable thing, and not in any way surprising. It was that turbulent region which seemed to him to be the real trouble in the way of making any further progress. Kármán had given a theory for the vortex wake far behind the body, and others were working and giving us some knowledge of the generation of vorticity at the body itself, but in between there was this blank turbulent region. That meant that, even though we knew what the strength of vorticity shed by the body was, we did not know what the strength of the vortex street would be, or its width, and one of the next steps to be taken was to connect the ultimate vortex street with the flow at the surface of the body.

Mr. E. F. Relf prefaced his remarks by pointing out that he ought not to discuss the paper at all, because he had collaborated in the work described and saw the matter very much from Mr. Fage's point of view. Referring, however, to Mr. Fage's discovery that if one wanted to make measurements of the frequency of eddies one should place the measuring instrument outside the street, he said that when he had tried to measure the frequency behind a cylinder some years ago he had placed the instrument in the path of the eddy cores, and had found considerable difficulty with the measurements. Another point of interest was Mr. Fage's proof that the motion was so steady at the commencement of the vortex sheets. That offered some hope to the coming generation of mathematicians, for if the motion had been unsteady there the problem would indeed have been almost hopeless from the mathematical point of view. He was very thankful that he and his colleagues at the laboratory were able to find time to

do this kind of work. It might be argued that it had no very direct application to aeronautics and, therefore, was not of primary urgency; but he considered that it was of great importance, because the more we understood the problem of fluid flow and knew what was happening behind bodies of various kinds, the better should we be in a position to forecast the future trend of research. We wanted to be able to obtain a mental picture of what was happening in the fluid, and Mr. Fage had given it for this one particular case. He must confess that before this work was done he would have said unhesitatingly that the position of the point at which the sheet left the body was moving to and fro in accordance with the periodic motion behind, and that there was a considerable periodic force acting sideways on the body. In this connection he remarked that he had twice been to a wind tunnel and hung up a stalled aerofoil on light springs, expecting to get a forced oscillation in the lift direction from which a rough estimate of the periodic lift variations could be made. The results were negative, and Mr. Fage's work explained why they should be. The flow near the aerofoil was steady; it was only at a considerable distance behind that there were discrete vortices, and these were alternatively of opposite sign, so that their effect in producing a periodic force on the body would be very small.

Mr. COWLEY said Mr. Relf's remarks had suggested one or two directions in which further investigations might be made. He had pointed out that the fact that the flow was steady round the body would help the mathematician. Mr. Fage had measured the various components of velocity, and it would appear that the information he had collected was sufficient to enable the equations of viscous flow to be checked. A point to be considered, however, was that although the motion was steady there, it was not altogether certain that it was two-dimensional. Mr. Fage had stated that it was, but Professor Taylor was of the opinion that, although it would appear that a flow of this nature was two-dimensional, actually the third dimensional portion of the flow had a very great bearing on the production of vorticity and eddies. Probably Mr. Fage had carried out some experiments by which he could settle that point. It is very important that the position should be cleared up and the checking of the viscous equations would help materially towards this end. He had been very much impressed by Mr. Fage's pictures of the width of the band of vorticity. Apparently the cases fell under two headings—those in which the band was comparatively narrow, and those in which it was fairly broad. Was it that in the case of the broad band there was a fairly low drag of the body, and in the case of the narrow band there was a higher drag? In the case of the narrow band the break away occurred comparatively near the nose, but in the case of the broad band the boundary layer seemed to cling more to the body and in consequence increase more in width. It might be that the width of the band throws some light on the question of whether the drag of the body is large or small.

Captain SAYERS suggested the substitution of the expression "vortex avenue" for "vortex street." It was unfortunate, he said, that the two words "sheet" and "street" should be used in phenomena so closely connected, and it was extremely easy for a misprint to occur and for uncertainty to arise as to which the author meant.

Mr. BRAMSON said that in the paper there did not appear to be any conclusions to be drawn from the observed facts. In thinking over these vortex phenomena one came to the conclusion that, after all, vortices in aerodynamics were "pathological" phenomena, and were things to be avoided, and in discussing them in detail it was difficult to overlook the fact that one was trying all the time to avoid them. He would like to get some information for a standard case as to the amount of energy per foot run contained in the vortex sheet or the vortex street. Another point that occurred to him was that in the illustrations given there was no indication of what, after all, must take place when an

aerofoil was dragged through the air, namely, the general downward acceleration which imparted to the general mass of air which it influenced, and the reaction which gave the lift. Finally, dealing with Professor Bairstow's reference to the desirability of reducing all these obscure phenomena to some simple theory which would cover all the observed facts, he said it seemed that, as there was an infinite variety of types of obstacle which it was possible to put into an airstream, it was very difficult to see how any simple theory could possibly render the service we wanted, namely, to enable us to predict what would happen.

## Reply

Mr. Fage, in reply to the discussion, thanked all the speakers for their contributions to the discussion and for their generous remarks on the paper. With regard to Professor Bairstow's remarks, the lecturer said that experiments of the kind described in the paper made one realise how little one knew about the subtleties of airflow. He thought, however, that although it was very difficult to subject the fundamentals of fluid motion to a close scrutiny, any attempts to obtain mental pictures of airflow were valuable, even if they were incomplete, because one then realised the difficulties involved, and when the difficulties assumed tangible form there was some possibility of solving them eventually. He agreed with Professor Bairstow that it was very important that an endeavour should be made to obtain the general solution of the differential equations of viscous fluid motion. Since, however, a direct frontal attack on these equations appeared to be a very formidable undertaking, recourse should be made whenever possible to indirect methods of attack, even if they were of limited scope, provided as Major Low had suggested, that the experimental measurements were co-ordinated by some sort of rational physical theory. With reference to a minor matter raised by Major Low that " stagnation point " did not appear to be a very suitable term, the lecturer stated that he had found it necessary to repeat several times the somewhat laborious experiments on the determination of the stagnation point, so that there were moments when the English translation of the German term " stau-punkt " had made a strong appeal to him. As Major Low had suggested, the areas of the vorticity diagrams in Fig. 9 should be equal, but discrepancies did exist and these were probably due to the difficulty of estimating with satisfactory accuracy the distribution of vorticity in a sheet of small breadth.

In reply to Dr. Piercy, the lecturer said that he and Mr. Johansen had recently completed some work at the N.P.L., which showed that Kutta-Joukowski relationship between lift and circulation held for an inclined flat plate (and also for a stalled aerofoil) immersed in an airstream, provided that the contour around which the circulation was taken was large and that it was drawn to cut the wake perpendicular to the direction of the undisturbed motion.

With regard to the effect of any lag in the response of the hot wire on the measurements of the velocity fluctuations, which had been mentioned by Dr. Richardson, the lecturer said that was a matter which had received consideration. Earlier experiments made at the N.P.L. by Messrs. Simmons and Lock had shown that a hot wire of diameter 0.001 in. faithfully recorded the amplitude of the velocity fluctuations provided that the frequency of the fluctuations did not exceed four a second; there was however a lag effect at higher frequencies. Accordingly, to minimise errors due to lag we used a hot wire of small diameter (0.001 in.) and made the experiments at a low wind speed (10 ft./sec.), the corresponding frequencies per sec. of the fluctuations being 2.92, 3.12 and 4.62 for incidences of the plate of 90°, 70° and 40° respectively. The lag in the response of the hot wire should not therefore appreciably influence the measurements of the amplitude of the velocity fluctuations. Further, a comparison of results taken at speeds below and above 10 ft./sec. showed that if there was an error due to lag, it was smaller than the

usual irregularities in the amplitude of the velocity fluctuations at a constant value of the mean speed ($V_0$). Other points raised by Dr. Richardson had been discussed by Mr. Glauert. The lecturer was in general agreement with the views expressed by Mr. Glauert, and would here thank him for directing attention to the importance of the interference of the tunnel walls on the drag of the plate and the characteristics of the vortex sheet trailing behind. This was also a suitable occasion to thank Mr. Relf for the interest he had shown whilst the experiments were in progress. The lecturer had found, as Mr. Sayers had remarked, that confusion did occasionally arise between the terms "vortex sheet" and "vortex street," and this would appear a matter which should be referred to the Nomenclature Committee of the Society.

In reply to Mr. Cowley, the lecturer said that the flow around a cross-section of a body of infinite span is commonly referred to as a "two-dimensional" flow, and in the paper he had used the term in that sense; he agreed with Mr. Cowley that the flow might be of a three-dimensional character. The breadth of a vortex sheet was dependent, *inter alia*, on the ratio of the area exposed to the airstream to the area of obstruction; the connection between the breadth of a sheet and the drag of the body was obscure. The lecturer disagreed with Mr. Bramson's remarks that there did not appear to be any conclusions to be drawn from the observed facts. In general, the results were interesting and should be known. We had got to make a start, and work of this sort was very suggestive as it progressed.

On the motion of the CHAIRMAN, a hearty vote of thanks was accorded Mr. Fage for his paper.

# SUMMARIES OF TECHNICAL PUBLICATIONS RECEIVED

PREPARED BY THE EDITOR.

*Wind Tunnel and Dropping Tests of Autogyro Models.* L. E. Caygill, B.Sc., A.M.I.M.E., and A. E. Woodward Nut, B.A. R. & M. No. 1116 (Ae. 289), H.M.S.O., 6d. November, 1926.

SUMMARY.

*Introductory.*—Wind tunnel and dropping tests under free flight conditions were carried out with models of autogyros to obtain further information, particularly as to their behaviour during vertical descent.

*Range of Investigation.*—Two model autogyros, one of 2.2 feet span and the other of 10 ft. span, were constructed and dropped from a height of 90 feet, records of rate of descent and speed of rotation of the vanes being taken. The lift and drag of the 2.2 ft. model were also measured in the wind tunnels.

*Conclusions.*—The values of normal force sufficient per unit disc area in vertical descent found in these tests for the two models were:—

2.2-foot model in seven-foot tunnel, 0.67 to 0.71.
2.2-foot model in free descent, about 0.6.
10.0-foot model in free descent, 0.58 to 0.71.

No evidence has been found of the very high value indicated by the full-scale demonstration flights.

*Further Developments.*—No further dropping tests proposed. Further wind tunnel tests are being made by the National Physical Laboratory, and further full-scale tests will be made by the Royal Aircraft Establishment.

*On the Influence of Supercharging on the Performance of Aeroplanes.* R. McKinnon Wood, M.B.E., B.A., A.M.Inst.C.E., F.R.Ae.S. R. & M. 1112 (Ae. 286), H.M.S.O., March, 1926. Price 9d.

SUMMARY.

This note discusses the improvement in the performance of an aeroplane which may be expected from supercharging the engine (*a*) when a fixed airscrew, (*b*) when a variable pitch airscrew, and (*c*) when a change speed gear is used. The question is discussed in a general theoretical way and diagrams are given to show the effects of supercharging on speed and climb at different heights, assuming typical data. The method of constructing these diagrams has been explained in some detail in order to assist the reader in drawing curves from different data.

It is concluded that supercharging involves a loss of performance at low heights, unless the engine be ground-boosted, and a great improvement at great heights. The variable airscrew is required to take full advantage of supercharging, but is not of very great value if the time taken to reach operating heights is unimportant, or if good performance is required at medium heights and not at very great heights. Supercharging may call for increased supply of fuel, depending on the purpose for which it is used.

*Closed Vessel Explosion of Carbon Monoxide, Oxygen and Nitrogen Mixtures.* R. W. Fenning, M.B.E., B.Sc., D.I.C. R. & M. No. 1113 (E. 26), H.M.S.O., January, 1927. Price 9d.

SUMMARY.

(*a*) *Reasons for Inquiry.*—The programme of work in connection with the N.P.L. closed vessel explosion apparatus includes the determination of the specific heat of gases at high temperatures.

(*b*) *Range of Investigation.*—The present report details experiments from which the dissociation of carbon dioxide at high temperatures can be computed.

(*c*) *Conclusions.*—The conclusions to be drawn from the experiments will be dealt with by Mr. H. T. Tizard, who has made an analysis of the results.

(*d*) *Further Developments.*—Experiments—in addition to those reported herein—are to be put in hand for the determination of the specific heat of $CO_2$ at high temperatures.

Six tables of results are given and two curves.

*An Electrical Indicator for High-Speed Internal Combustion Engines.* J. Obata and Y. Yosida. Report of the Aeronautical Research Institute, Tokyo Imperial University, No. 28, December, 1927.

The indicator described in this paper is, in principle, nothing but the simple disc form. The only difference from the familiar mechanical or optical ones lies in the use of a very thick steel disc, two mm. thick and five cm. in diameter, and of an extremely sensitive electrical means, known as the "ultra-micrometer method," utilising a generating valve circuit for recording the minute motion of the thick disc caused by the pressure of the cylinder.

Using two small engines several records of actual engine pressure were obtained by means of an Einthoven string-galvanometer, a Lutz-Edelmann string-electrometer and a Duddell oscillograph, and it is found that the indicator is suitable for high-speed engines, with which ordinary indicators fail to give correct diagrams.

*On Preparation of Lead Tetraethyl.* By Y. Tanaka and T. Kuwata. Report of the Aeronautical Research Institute, Tokyo Imperial University, No. 29, December, 1927.

ABSTRACT.

Lead tetraethyl, known as a powerful anti-knock material, may be prepared by the reaction of ethyl magnesium iodide or bromide (Grignard's reagent) with lead chloride. In this method it is known that ethyl iodide or ethyl bromide is a good starting material, whereas ethyl chloride cannot be used for the sake of very weak activity.

The authors have found that an ethereal solution containing ethyl iodide (or bromide) and iodine was a suitable catalyser for the reaction between ethyl chloride and magnesium. When the solution was added to magnesium, ethyl magnesium iodide was instantaneously formed and the latter accelerates catalytically the reaction between ethyl chloride and magnesium. After the reaction once started between ethyl chloride and magnesium, the subsequent reaction is accelerated by ethyl magnesium chloride formed. Then the crude lead tetraethyl may be readily prepared by the reaction of ethyl magnesium chloride and lead chloride.

The crude lead tetraethyl containing a considerable proportion of unsaturated lead triethyl was treated with air or oxygen in presence of dilute hydrochloric acid, by which lead tetraethyl remained unchanged, and lead triethyl was con-

verted into a crystalline mass of triethyl lead chloride. The latter was then transformed into lead tetraethyl by the action of ethyl magnesium chloride, with the yield of 95 per cent. of the theory. The total yield of lead tetraethyl to the amount of ethyl chloride used was approximately 74 per cent. of the theory.

*Wind Tunnel Tests on Auto-Rotation and the Flat Spin.* M. Knight. N.A.C.A. Report No. 273. Washington, 1927.

SUMMARY.

The report deals with the auto-rotational characteristics of certain differing wing systems as determined from wind tunnel tests made at the Langley Memorial Aeronautical Laboratory. The investigation was confined to auto-rotation about a fixed axis in the plane of symmetry and parallel to the wind direction. Analysis of the tests leads to the following conclusions:—

Auto-rotation below 30° angle of attack is governed chiefly by wing profile, and above that angle by wing arrangement.

The strip method of auto-rotation analysis gives uncertain results between maximum $C_L$ and 35°.

The polar curve of a wing system, and to a lower degree of accuracy the polar curve of a complete airplane model are sufficient for direct determination of the limits of rotary instability, subject to strip method limitations.

The results of the investigation indicate that in free flight a monoplane is incapable of flat spinning, whereas an unstaggered biplane has inherent flat-spinning tendencies.

The difficulty of maintaining equilibrium in stalled flight is due primarily to rotary instability, a rapid change from stability to instability occurring as the angle of maximum lift is exceeded.

*The N.A.C.A. Photographic Apparatus for Studying Fuel Sprays from Oil Engine Injection Valves and Test Results from several Researches.* E. G. Beardsley. N.A.C.A. Report No. 274. Washington, 1927.

SUMMARY.

The apparatus consists of a high-tension transformer by means of which a bank of condensers is charged to a high voltage. The controlled discharge of these condensers in sequence, at a rate of several thousands per second, produces electric sparks of sufficient intensity to illuminate the moving spray for photographing. The sprays are injected from various types of valves into a chamber containing gases at pressures up to 600 lbs. per sq. in. The results give the effects of injection pressure, chamber pressure, specific gravity of the fuel oil used, and injection valve design, upon spray characteristics.

*The Comparative Performance of an Aviation Engine at Normal and High Inlet Air Temperatures.* A. W. Gardiner and O. W. Schey. N.A.C.A. Report No. 277. Washington, 1927.

SUMMARY.

Tests were carried out on a Liberty 12 aviation engine to ascertain, for normal service carburetter adjustments and a fixed ignition advance, the relation between power and temperature for the range of carburetter air temperatures that may be encountered when supercharging to sea-level pressure at altitudes of over 20,000 feet and without inter-cooling when using plain aviation gasoline and mixtures of benzol and gasoline.

Laboratory tests were made at full throttle over the speed range from 1400 to 1800 r.p.m., in which the pressure at the carburetter and exhaust was main-

tained sensibly constant and the inlet air temperature varied from 45° to 180°F. The range of mixtures was that normally used in flight. Plain aviation gasoline, a mixture consisting of 30 per cent. (by volume) of commercial benzole and 70 per cent. gasoline, and a mixture of 65 per cent. benzol and 35 per cent. gasoline was used. Additional tests were made with a Wright E.4 aviation engine.

The results show that for the conditions of test, both the brake and indicated horse-power decrease with increase in air temperature at a faster rate than given by the theoretical assumption that power varies inversely as the square root of the absolute temperature.

*Lift, Drag and Elevator Hinge Moments of Handley Page Control Surfaces.* R. H. Smith. N.A.C.A. Report No. 278. Washington, 1927.

SUMMARY.

This report combines the wind tunnel tests on four control surface models made in the two wind tunnels of the Navy Aerodynamic Laboratory, Washington Navy Yard, during the years 1922 and 1924. The purpose of the tests was to compare, first, the lifts and the aerodynamic efficiencies of the control surfaces from which their relative effectiveness as tailplanes could be determined; then the elevator hinge moments upon which their relative ease of operation depended. The lift and drag forces on the control surface models were obtained for various stabiliser angles and elevator settings in the 8 by 8-foot tunnel by the writer in 1922; the corresponding hinge moments were found in the 4 by 4-foot tunnel by Mr. R. M. Bear in 1924.

*Helium Tables.* Lieut.-Cmdr. C. H. Havill, U.S.N. N.A.C.A. Technical Note No. 276. Washington, January 1928.

SUMMARY.

These tables are intended to provide a standard method and to facilitate the calculation of the quantity of "standard helium" in high pressure containers. Tables of the lift factors of helium are given under varying conditions of pressure and humidity.

*Special Propeller Protractor.* A. L. Heim. N.A.C.A. Technical Note No. 272. Washington, January, 1928.

SUMMARY.

Lack of equipment suitable for measuring aeroplane propellers at flying fields and storage depots prevents the accumulation of reliable information on the effect of storage and service on the working and change of blade angles in propellers. A special protractor has been designed with a view toward supplying a simple, inexpensive, practical, portable instrument for making these measurements under practically all conditions and without the use of auxiliary equipment. The instrument and its use are described.

*Full-Scale Drag Tests on various parts of Sperry Messenger Airplane.* F. E. Weick. N.A.C.A. Technical Note No. 271. Washington, January, 1928.

SUMMARY.

The drag of a Sperry Messenger airplane with the wings removed, and also the drag of its various component parts, was measured in the 20ft. air stream of the N.A.C.A. propeller research tunnel at air speeds from 50 to 100 m.p.h. It was found that the three-cylinder radial air-cooled engine nearly doubled the drag of the bare fuselage, and the drag of the landing gear was about the same as that of the fuselage and engine combined.

*Wireless Direction-Finding and Directional Reception.* R. Keen. Iliffe & Sons, Ltd., London. 21/- net.

CHAPTER HEADINGS.

Introduction; Directional Properties of Aerial Systems; Frame Aerial Reception; Rotating Frame D.F. Installations; The Bellini-Tosi System; Maps; Position Finding and Wireless Navigation; Beacon Transmitters; Night Effect and other Freak Phenomena; The Shore D.F. Installation; The Ship D.F. Installation; The Aircraft D.F. Installation; Fault Clearing and Maintenance; Notes on Field and Nautical Astronomy; Bibliography.

*The Journal of the Institute of Metals.* Vol. XXXVIII., No. 2, 1927. London, 1927.

TITLES OF PROCEEDINGS REPORTED.

Synopsis of a paper on Minute Shrinkage Cavities in some Cast Alloys of Heterogeneous Structure, W. A. Cowan, B.A.; Non-Ferrous Metals in Modern Transport, Leslie Aitchison, D.Met., B.Sc., F.I.C.; The Constitution of Alloys of Aluminium with Silicon and Iron, A. G. C. Gwyer, B.Sc., Ph.D., and H. W. L. Phillips, B.A., A.I.C.; Grain Growth in Compressed Metal Powder, C. T. Smithells, D.Sc., W. R. Pitkin and J. W. Avery, B.Sc., D.I.C.; The Copper Magnesium Alloys, Part II., W. T. Cook, B.Sc., A.I.C., and W. R. D. Jones, M.Sc.; Researches on Inter-Metallic Compounds, VI., The Reaction between Solid Magnesium and Liquid Tin, W. Hume-Rothery, M.A., Ph.D.; Note on Cathodic Disintegration as a Method of Etching Specimens for Metallography, C. S. Smith, B.Sc. (Hons.), D.Sc.; The System Magnesium-Cadmium, W. Hume-Rothery, M.A., Ph.D., and Sydney W. Rowell, B.A.; The Under-Cooling of some Magnesium Alloys, M. L. V. Gayler, D.Sc.; Age-Hardening Tests with Elektron Alloys, K. L. Meissner, Dr.Ing.; The Equilibrium Diagram of Copper-Tin Alloys containing from 10 to 25 atomic per cent. of Tin, A. R. Raper, B.A.; The Protection of Aluminium and its Alloys against Corrosion, N. Sutton, M.Sc., and A. J. Sidery, Assoc.Met.; The Nature of the Film produced by Anodic Oxidation of Aluminium, H. Sutton, M.Sc., and J. W. W. Willstrop, B.Sc., A.I.C.; The Constitution and Physical Properties of some of the Alloys of Copper, Zinc and Cadmium, C. H. M. Jenkins, B.Sc., A.R.S.M.; Effect of Work and Annealing on the Lead-Tin Eutectic, F. Hargreaves, A.R.S.M., D.I.C., F.I.C.

*The Junior Institution of Engineers, Journal and Record of Transactions.* Vol. XXXVII., 1926-27. London, 1927.

TITLES OF PROCEEDINGS REPORTED.

*(Only those of interest to Aeronautical Engineers are given.)*

An Investigation of Torsional Vibration, with particular reference to Aircraft Engines, J. Calderwood, M.Sc.; Distortion in Wireless Reception, A. Ballhatchet; Short Wave Wireless Communication, B. J. Axten, A.M.I.R.E.; Survey of Modern Airless Injection Oil Engines, N. J. Griffin.

## BRANCH NOTES

### *Coventry Branch*

The lecture by Major F. M. Green, O.B.E., on A Trip to South Africa, delivered on Tuesday, February 28th, under the chairmanship of J. J. D. Siddeley, Esq., C.B.E., was extremely well attended. Major Green gave a most interesting lecture dealing with his experiences and illustrated it with many personal and unique photographs and a cinematograph film taken by himself. On March 9th a film The Age of Speed was shown followed by a discussion, and on March 23rd Mr. Stuart Tegner gave a lecture on Drilling for Oil and the Production of Petrol, Major Green being in the chair. The lecture was illustrated by a film and a discussion followed during which the value of ethyl spirit was considered. Those who could speak with authority and knowledge expressed the opinion that ethyl spirit was of value in engines using very high compression ratios.

### *Westland Branch*

Lectures were given in January by Squadron Leader H. M. Probyn on Some Experiences with Aircraft in the Near East; Major C. J. Stewart on Modern Developments in Aircraft Instruments; Mr. R. J. Norton on Sir Henry Bessemer and his work. In February, Mr. St. John Plevins on Drilling for Oil and the Production of Petrol; Mr. H. L. Stevens on Performance Testing of Aircraft. And in March by Mr. H. H. Stoodley on Acetylene Welding; Mr. F. Radcliffe on High Speed Aircraft; Flight-Lieut. S. N. Webster, A.F.C., on The Schneider Trophy Contest of 1927; Mr. W. Jeffries on Jupiter Aero Engines and Mr. R. C. Taylor on Inspection of Aircraft. It is hoped to print some of these lectures in future issues of the Journal.

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## REVIEW

### *Report of the Daniel Guggenheim Fund for the Promotion of Aeronautics,* 1926 *and* 1927

The Daniel Guggenheim Fund for the Promotion of Aeronautics was formed in January, 1926. In two years it has obtained a world-wide reputation and in this, its first report, it is shown that this reputation has been deservedly won. It is safe to say that the Daniel Guggenheim Fund has done more to stimulate interest in aviation than anything since flying became an accomplished fact.

The first objective of the fund is the development of safety in aviation, and it has organised a Safe Aircraft competition which has attracted entries from all parts of the world. The result of this competition on the future of aviation will undoubtedly be profound and will, in itself, draw the attention of the man in the street to new forms of transport which are rapidly conquering spaces presenting insuperable difficulties to the older forms of transport.

The fund has been lavish in generous contributions to stimulate interest in the study of aeronautical science, to provide means for the ever-increasing necessity for research work, and for the dissemination of technical information. In all it has given the large sum of $808,000 to various universities and societies, and one records with gratitude that the fund has helped the work of the Royal Aeronautical Society in two successive years by generous grants totalling $10,000.

Every side of aeronautics is touched upon by the fund, from those outlined above to meteorology and navigation, to the financing of the Commander Richard E. Byrd and Colonel Lindbergh tours. To Mr. Daniel Guggenheim aviation all over the world owes a debt of gratitude which it will be difficult ever to repay.